DISCOVERY

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DISCOVERY

THE MAGAZINE OF SCIENTIFIC PROGRESS

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The Progress of Science

This way to the Tomb!

NEVER in the whole history of the world has there been a time more crucial than the present. Never has there been a greater opportunity, nor a more imperative duty, for the voice of science to speak with unity, strength and practical vision than there is now. Never has there been a disappointment so bitter at the failure, complete save for a few shining exceptions, of the scientists and the scientific organisations of this country to voice a concerted view on the question of the international control of the atomic bomb, and of atomic energy.

Almost as soon as the atomic bomb had been dropped a considerable section of opinion in this country began to take up a rather critical attitude towards the United States. The question was asked whether the Japanese War could not have been ended without dropping the bomb on Japanese cities, and it was maliciously suggested that the fact that action was taken in this form indicated a predominance of the sort of "racial" opinion which regarded the Japanese as subhumans, unworthy to be accorded the privileges of the palefaces. The wiseacres shook their heads, and remarked, with fatuous knowledgeability, that they had guessed, from reading pre-war scientific literature, that something of the kind was on the way. The political amateurs discussed the probable attitude of American Big Business. They pointed out, again quite correctly, that the lack of control of American economy by State intervention was likely in the future to lead to internal difficulties, and the development of an urge towards economic imperialism. The amateur strategists predicted friction and a possible future conflict between the United States and the U.S.S.R. and they discussed methods of rendering the atomic bomb ineffective.

Oh, yes! In Britain we are politically awake; we don't live on an ivory tower. We can all see with perfect clarity exactly what is going to happen. We are all advocates of international control of atomic energy production, and we are all sufficiently hard-headed and realistic not to have the slightest faith that international measures of control will be in the least effective. We get together over coffee, scientists and progressive thinkers alike; we have little meetings, and explain to each other how foolish the politicians are. We tell each other a great many things that the general public has not yet realised; our gravity is a terrible thing to see, and our mutual admiration at our own perspicacity even more so. And all the time we are just ever such a bit superior about the idealism and political naiveté of the Americans.

And now let us see just how these colonial savages have made us look silly. American scientists led off by pointing out that the secret of atomic energy could not be permanently kept and urged publication and control by the duly constituted international Security Council. The stand they took was as firm as a rock. Senatorial opinion clustered itself into three groups. One wholly agreed with the scientists. The second agreed with the scientists that permanent secrecy was not possible but urged that America should secure what temporary advantage it could from its present advantageous position. The third group took the line that America would no more share its technical knowledge of atomic energy generation than it would share its Navy with other nations. Nobody mentioned any nation by name but there is no doubt which one was particularly in mind. That was at the end of September. At the beginning of October members of the House of Representatives urged President Truman to keep the secrets in the control of a commission consisting of scientists, the Chiefs of Staff, the State Department and Congress. By the middle of October the American scientists had seen which way the wind was blowing, and had acted with vigour and decision.

Four hundred of them had formed an Association of Los Alamos Scientists and had issued a public statement in which they asserted that any attempt to keep the secrets would lead to "*an unending war, more savage than the last.*" They pointed out that countries other than the United States, Great Britain and Canada could manufacture bombs, perhaps thousands of times more powerful than those already manufactured. They asserted that counter-measures would be extremely difficult and that all the advantage would lie with the aggressor. They believed that control would be technically feasible and said "*The actualities of the situation require a drastic solution. . .*

THE ATOM-SQUATTERS

"WHY CAN'T WE WORK TOGETHER IN MUTUAL TRUST & CONFIDENCE?"

(Copyright in All Countries)

. . . He put in his thumb,
And he took out a plum,
And said, " What a good boy am I !"

The world has entered the Atomic Age, but international statesmanship lags behind in the horse-and-buggy period. These cartoons are representative of British opinion on the present lack of an international policy. (*Reproduced by courtesy of the editors of the Daily Mail, Evening Standard and News Chronicle.*)

The lack of a decision within a even few months will be preparing the world for unprecedented destruction, not only of other countries, but of our own as well."

Meanwhile, the Naval Affairs Committee of the House of Representatives (fearful perhaps of naval unemployment before the next war should start?) had thrown out an optimistic statement to the effect that the development of electronics had already led to a counter-measure by which atomic bombs could be exploded far short of their objective. The scientists in Chicago were not found wanting either. A newly formed group, the Atomic Scientists of Chicago, contradicted this report, and said that the impression that the armed forces could soon bring the situation under control would do incalculable harm.

By the 18th of the month President Truman's message to Congress urging the retention of secrets by the United States was being considered by the House Military Affairs Committee, preliminary to the drafting of legislation. (President Truman's statement of October 27 underlined that message.) The scientists telegraphed their opposition, and asserted that control by a commission such as that suggested by the President would be most damaging to the progress of research and the peaceful utilisation of atomic energy. Dr. J. R. Oppenheimer (formerly head of the Los Alamos Laboratory) appeared before the Senate Committee and scoffed at the idea of counter-measures such as had been suggested by the Naval Affairs Committee. The only defence, he said, is international organisation. Our bombs are not explodable at a distance, and there is no counter-measure unless you can learn to shoot down a V2-type rocket. Dr. H. J. Curtis, representing the scientists at Oak Ridge, stated publicly that international organisation was also the only solution to the secrecy problem.

There we have it: these naïve, unpolitical scientists have got together. They all say the same thing. They say it loudly and clearly for everyone to hear. They are moulding public opinion and mobilising it in support of their attitude. They are in a mood approaching open revolt against their Government. They have courage and guts, as Professor Oliphant has said, and they put their British colleagues to shame.

And what is happening here? The right-wing scientists are calling proposals to put all information in the control of the Security Council "idealistic" and meanwhile are hoping, like Mr. Micawber, for a growth in man's moral stature. The left-wing scientists who in the past have been very superior about their "conservative" or "reactionary"

colleagues, are now making every mistake which the most perverted ingenuity could devise. When they should be guiding public opinion, which cannot be expected to judge the complex issues without such aid, they are holding meetings in private. When they should be unanimous they are divided. When they should throw political theory to the winds and talk straight sense in public, they are busy being sectarian. When they should be quick, as our American colleagues have been, they are intolerably slow.

A statement by the Association of Scientific Workers has just come to hand, too late to discuss it in this issue of DISCOVERY. This statement has so far attracted little public attention, but it is important nevertheless, being the first expression of collective opinion to come from British Science. One hopes the call it contains for a meeting of scientific bodies early in 1946 will meet with a ready response.

It must be conceded that the situation here is a little more difficult than in the United States. There are far fewer atomic bomb scientists here, and because of the effects of the shortage of scientific manpower in 1940 a considerable number of those who have been concerned in the work in Britain are not of British birth. It is peculiarly difficult for these men to make any sort of firm public stand (the difficulty is aggravated by the service of some of them on Government committees) but, in consequence, the duty devolves not only on the British-born scientists actually concerned but on the scientific profession as a whole. It is not necessary to be in possession of *all* the inside information in this instance to be able to form a definite opinion. For those who are not physicists the stand should be on the ground of scientific principle: for those who are physicists a thorough knowledge of the published facts and an experience of the working out of scientific applications is all that is needed as a basis for constructive action. Priding ourselves as we did on the existence of a large body of progressive scientific opinion in this country, we should have expected to have seen the emergence of solid support for our American colleagues long ago.

As it is, only a few scientists—Professor Oliphant of Birmingham, waging a one-man campaign to enlighten the public, Professor A. V. Hill, speaking at the Parliamentary and Scientific Committee's lunch early in October, and almost nobody else—have done more than repeat the obvious. Hardly anyone has given the sort of lead which is essential and which is demanded by the extreme urgency of the situation. Our trouble is that we are all too dignified and too ready to delegate action to others. We speak of the magnificent heritage of science, we condemn the German scientists who could not stick it out alone against the Gestapo, but it begins to look as if scientists in Britain have been so overcome by the fetish of secrecy and security that it has got into their blood and driven out the overriding duty to speak the truth though the heavens fall. We must bring to that duty some fraction of the courage and devotion displayed by the ordinary citizen, whom we see from our comfortable cinema seat, dying in real earnest on the screen to save us from Fascism. If we can do that, then we may have some right to call ourselves the heirs of men like Giordano Bruno and the other martyrs of science who felt in their own bodies the retribution for service to the spirit of science and made a glorious end in humiliation and indignity.

We have become accustomed to say in reply to the accusation that science is motivated by evil in its service to war, that the responsibility for the use of science lies with the community that pays for the science. To take refuge in this now is to fail in a duty. Dr. Oppenheimer knows that President Truman and his political advisers know less about this business than he does, and he has given proof of his determination not to be made a monkey by them. (It is insufficiently realised that the politicians had little or no faith in the atomic bomb project, which they looked upon as a scientific pipe dream, unlikely to lead to any material result. So that whereas the scientists have been thinking about atomic energy and its implications for the past three years or so, politicians did not start thinking about it until this summer, when the test bomb was exploded, to prove just how shattering a scientist's pipe dream can be. Small wonder that the newspaper columnist, Don Iddon, can remark, "The debate on atomic power continues and its note is becoming shrill and almost hysterical—except when the scientists speak.")

We ought also to recognise that speeches by eminent industrialists on the future of atomic power are second-hand stuff, whereas appeals for international control and an end of secrecy by such men as Gilbert Murray, who is doing the job that scientists ought to be doing themselves, could be made more effective if supported by scientists and technicians. In this instance there is a definite responsibility and, if it is evaded, all the accusations about the wickedness of scientists, their inhumanity and indifference, and all the rest, will be justified. Now is the time when the claim for the internationalism of science will stand or fall. Where does duty lie? Is it to the eternal principles of science which we have paraded in the past, or to the power of our own nation? If our claim is a true one, then the scientists must stand together, and if in making a stand, accusations of lack of patriotism can be laid, then the stand must be made in public and must be publicly explained so that the judgment of the world may be freely given. And if there must be martyrs for a worthy cause, and there may have to be, let the thing be done publicly, so that they shall not stand alone, nor suffer in vain.

Hormones and Weedkillers

PLANT hormones have never attracted public attention in the way that the animal hormones have. The auxin theory of plant growth is at least as fascinating as the story of insulin, yet it is hardly known to non-scientists. The general public are aware that diabetes is connected with a failure of the pancreas to secrete insulin; how many laymen who know about the vernalisation of cereals are aware that a hormonal theory has been formulated which neatly explains the phenomenon?

Admittedly there is to-day quite a large section of the pharmaceutical chemical industry concerned with extraction and synthesis of animal hormones, and a large amount of money is spent on advertising its achievements and its claims. Further, it has to be recognised that animal hormones have a direct interest to the lay mind, preoccupied as it is with fears of the economic consequences of ill health and its almost psychopathic curiosity in the

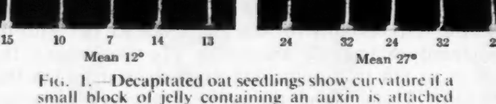

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FIG. 1.—Decapitated oat seedlings show curvature if a small block of jelly containing an auxin is attached eccentrically. The five seedlings on the left have responded to indolyl propionic acid, those on the right to indolyl acetic acid (heteroauxin); in the first batch there is a short zone of curvature, whereas the heteroauxin produces curvature over a greater distance. Using this delicate technique, one milligram of heteroauxin proves sufficient to cause "standard curvature" (10° curvature in 2 hours at 22° C) in 30,000,000 oat seedlings (*from "Phytohormones" by F. W. Went and Thimann*).

physiology, both normal and pathological, of the human body, a curiosity that tends to topple over into hypochondria because of the lack of a sensible biological education in our schools. Plant hormones, on the other hand, have no such direct hold on public interest though indirectly they may soon come to have a direct bearing on nutrition if the world implements an expansionist food policy, which men like Sir John Orr have advocated through boom and slump and which the United Nations Food and Agricultural Organisation has taken as its lodestar. For hormones offer us a chance to influence the physiology of crop plants to our advantage, while presenting us with a new method of controlling weeds.

The study of plant hormones is no new subject. Scientists postulated the existence of such substances many years ago, but it was not until the first quarter of this century that systematic experiments were begun which showed that hormones did, in fact, occur. In 1917, for instance, it was found that the water fern *Azolla* and the duckweed *Lemna* did not thrive when grown on solutions containing all the chemical elements then considered to be essential to plant growth. Later the technique of the particular experiments from which this observation derived was challenged, further work having led to the somewhat different view that these plants could grow on the so-called complete culture solution but that their growth could be stimulated by the addition of a minute amount of organic matter. The increase in growth rate so caused seemed to be out of all proportion to the amount of body-building elements present in the organic material added. Professor Ashby and others showed that a mere speck of horse dung added to the culture solution had this effect; the addition of 0·2 parts of dung to 1,000,000 parts of solution caused a measurable difference. This was one line of research that led scientists to believe in the existence of growth-promoting substances, though the concept that came out of these experiments was of vitamin-like substances rather than of hormones.

Another line of research (the two lines were to converge later) had its origins in the attempts to find an explanation of how plants responded to stimuli by growth, an explanation of tropisms as they are called. (An example of a tropism is *geotropism*; an ordinary plant, say a lupin or a geranium, exhibits geotropism strikingly if it is placed so that stem and root lie horizontally; by growth both stem and root are brought into the normal vertical position, the response to the stimulus of gravity being called geotropism.) In 1919 the Danish worker Boysen-Jensen proved the existence of growth-promoting substances by means of a series of very neat experiments. His experimental material was oat seedlings. The growth of the seedling "stem" (more accurately called a *coleoptile*) depends upon cell division and cell elongation and the region where this occurs lies at some distance from the tip. With a very sharp razor it is possible to decapitate the oat seedling "stem" without causing serious damage to the tissues of the tip and the stump that remains. After decapitation the stump continues to grow, but the growth rate is less than that of a normal seedling. The tip can be stuck back on the stump by means of a dab of gelatine (extreme delicacy in handling is naturally essential to success here) and then the growth rate is found to be restored almost to normal. This suggested the presence in the tip of some growth-promoting substance, an auxin, that moves down the stem by diffusion, and this hypothesis was substantiated by the results of further researches.

The auxins can be isolated from the plant; if a number of oat seedling tips are placed on gelatine and auxin diffuses out into the gelatine, a small block of which causes curvature of a decapitated stem if applied eccentrically (Fig. 1). Extracted in this manner these auxins prove not to be specific; the auxin from maize seedlings will affect the growth of oat seedlings and *vice versa*; similarly the auxin extracted from a root is effective on stems, though in the root the auxin appears to *reduce* the growth rate whereas it has an accelerating effect on the stem. The curvature just mentioned is due to more rapid growth on one side.

The auxins proved to be thermostable substances,

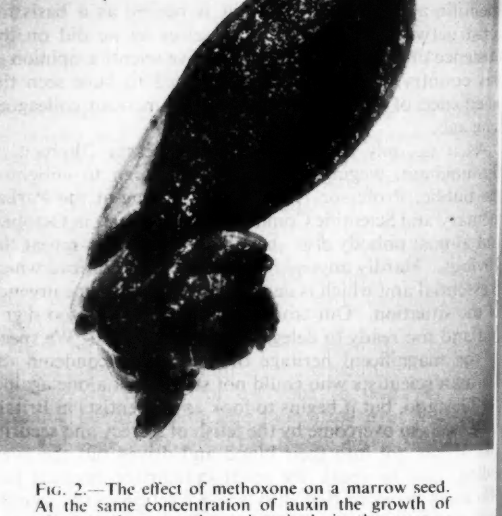

FIG. 2.—The effect of methoxone on a marrow seed. At the same concentration of auxin the growth of wheat seeds was quite unimpaired; in the marrow the root became contorted and growth disturbed to the extent that the seed leaves never completely emerged from the seed coat. This emphasises the point that methoxone must not be used indiscriminately; on the other hand it needs to be understood that there would be no danger to a crop like marrow or cabbage following a crop of methoxone-treated wheat, for the substance is soon washed out of the soil.

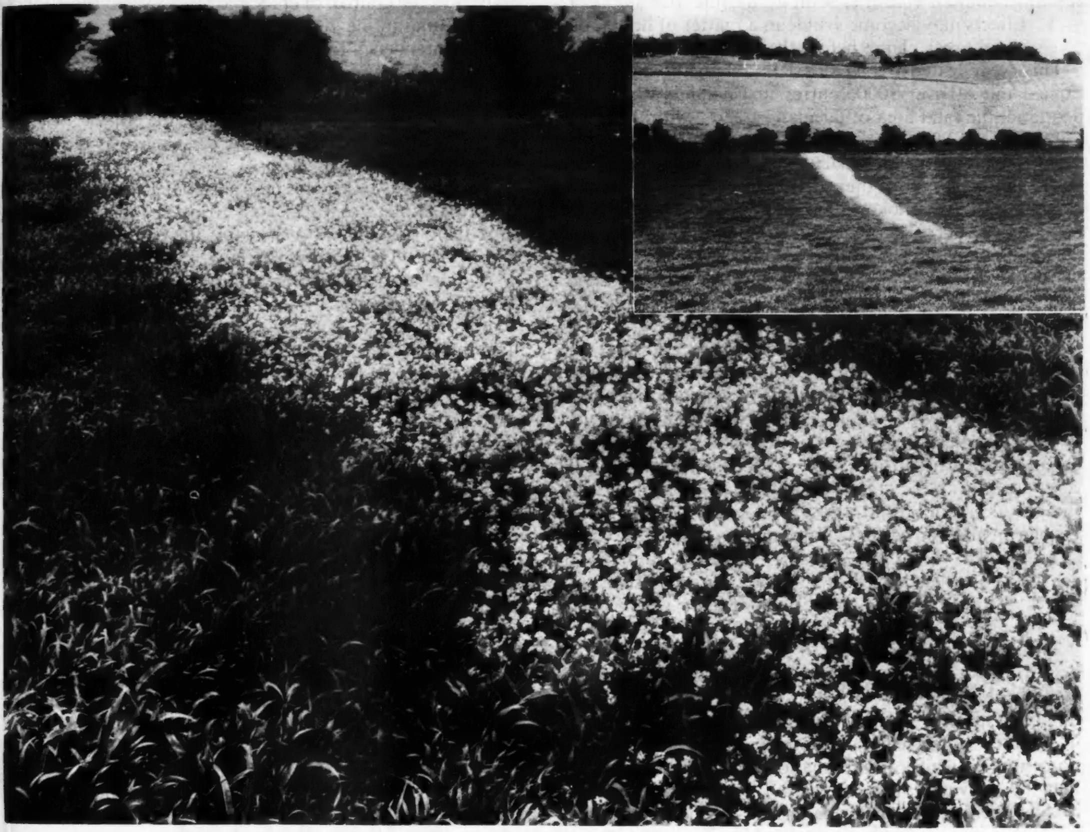

FIG. 3. (*inset*).—The field in the foreground has been treated with methoxone, in powdered form. Wherever the methoxone fell only the wheat has come up. One strip received no methoxone and there the charlock can be seen in flower. The very definite line of demarcation between the treated and untreated portions is well seen in the large photograph.

soluble in water and ether. Their velocity of diffusion was comparable to that of chemical substances with molecular weights around 370. Soon afterwards, chemicals with molecular weights of that order were isolated from horses' urine. The first of these to be discovered was *auxin a* (auxentriolic scid, $C_{18}H_{32}O_5$); then came *auxin b* (auxenolonic acid, $C_{18}H_{30}O_4$). These substances were found to have the same physiological effect as the auxin of oat seedlings, as did a third chemical compound, β-indolyl acetic acid ($C_{10}H_9O_2N$) or *heteroauxin*. These chemicals can be prepared synthetically. A related compound, α-naphthylacetic acid, is the basis of "Hortomone" and other commecial preparations used to hasten the rooting of cuttings, and to prevent pre-harvest drop of apples. (There was a striking demonstration of the use of this substance to produce seedless, early-ripening tomatoes, in the Plant Physiology Laboratory of Imperial College which visitors were shown on the occasion of last month's centenary celebrations.)

Arising from the pre-war synthesis of plant hormones it was discovered in 1940 that at certain concentrations α-naphthylacetic acid arrested or retarded seed germination and the early growth of some annual weeds without affecting cereals. Many related compounds were prepared and investigated and finally methoxone (4-chloro-2-methylphenoxyacetic acid) was found to be outstandingly active. Applied in water solution or in dust at rates as low as 8 ounces an acre it killed weeds such as yellow and white charlock, corn buttercup, creeping buttercup and pennycress without damaging the cereal crop. Other weeds of wheatland were seriously affected, including corn marigold, fat hen, speedwell, chickweed, poppy and sow thistle. A few weeds were unaffected, such as bladder campion, coltsfoot, and goosegrass. Methoxone has a distinctive and markedly selective action, the mechanism of which is not yet fully understood. It is absorbed into the weed through the leaves and through the root, to cause serious physiological disturbance (Fig. 2). Growth is arrested; the stem and leaves become twisted and contorted, the stem thickens, and may split to produce a mass of adventitious rootlets. The foliage changes colour and finally the plant dies. The process is slow compared with the

action of sodium chlorate, sodium arsenite and sulphuric acid. Effects may become visible in a matter of hours, but several weeks may elapse before the weeds die.

This year extensive field trials of methoxone were carried out at over 1000 centres in England, Wales and Scotland, the total area of farmland treated with the weed-killer being 13,000 acres. Wheat and oats showed no trace of damage: with barley 4 cases out of 63 showed slight malformation of the ears (Fig. 3).

Botanically, methoxone has an interest that goes far beyond its agricultural application. Antibiotics, like penicillin, had already started plant ecologists thinking about the way in which plants compete with each other. One of Professor Salisbury's workers, S. C. Varma, has produced evidence that the depression of root growth in competition is due to soluble substances and that the effects of these vary with the plant species concerned. (The depressive effect that the roots of one plant can have on the root growth of another was strikingly brought out in some estimates made by the Canadian botanist, Pavlychenko, for cereals; for a single wheat plant grown on its own, no other vegetation being near, the total length of the roots amount to the staggering figure of 45 miles. Drilled in the conventional farming way, the figure drops to one-half or one-third of a mile, while the presence of charlock in a field of wheat reduces the root system to a length one-fifth or one-tenth of its extent when weeds are absent.)

Profitable speculation as to how fungi productive of penicillin-like substances may compete and give rise to distinctive successions of species has already started; the different species of dung fungi that follow each other in definite sequence is an example of the kind of phenomenon for which an explanation, on antibiotic grounds, may be provided. An interesting new field of research is thus opening up, quite apart from all the development work that can be done to elucidate the mode of action of methoxone and other plant hormones, to find new applications for known substances and to synthesise new substances with different properties. The separate research sectors are beginning to mesh, and mutual stimulation of research on antibiotics, hormones and ecology is inevitable and profitable.

The Control of Epidemics

It is often pointed out that the last war led to more deaths by disease than by military action. We have so far been fortunate to escape any serious epidemics arising out of this war, but our fortune is due not to accident but to the vigilance and forethought of medical statesmanship. In a recent issue of the Proceedings of the Royal Society of Medicine Dr. P. G. Stock gives an account of the International Sanitary Convention of 1944 which came into force early this year, and which is only the last of a long series of international agreements designed to control the ravages of epidemics. Though much of the account is intended for the medical man and consists of a detailed description of regulations and control measures, Dr. Stock enlivens his description with historical references. The oldest in point of time refers to the devastating results of the introduction of plague into Byzantium by corn ships from Egypt in the year 543 when Justinian was Emperor.

That part of the world has been the scene of much activity in the control of epidemic diseases and the application of quarantine regulations, the reason being apparently the tendency to the spread of disease by pilgrims to Mecca and elsewhere. One control body was the Constantinople Board of Health which had a semi-international status and controlled the ports of the Black Sea and Dardanelles, and whose separate existence was terminated in 1923.

The stimulus to general international co-operation came from the cholera outbreak in Europe in 1848-50 at which time there were 72,000 victims of the disease in England and Wales. The first international sanitary conference of 1851, convened by France as a result of this, was not a success, and it was only in the early years of the present century that effective international action became a reality. The International Sanitary Convention of 1903 dealt with cholera and plague, and recognised the part played by rats and their fleas in the case of the latter disease. From this came the Office Internationale d'Hygiene Publique established in 1909 in Paris, a parallel organisation to the Pan-American Sanitary Bureau which had been established in Washington in 1902.

The Convention of 1944 is a modification of the 1926 convention which prescribed measures for the international notification, control of and exchange of information on five "Convention" diseases—plague, cholera, yellow fever, epidemic typhus and smallpox.

Some years after this was ratified it became increasingly clear that rapid air travel held a menace to health by the possible transmission of infection or the unintentional transport of insect disease carriers. An international agreement signed in 1933 was rather less successful than its prototype of 1926 and, for example, only four of the western hemisphere nations signed it.

In all these conventions the principle adopted has been to prescribe maximum precautions which interfere as little as possible with rapid transit of passengers and to rely on swift detection of infection.

The important work of epidemic control which must be undertaken in devastated Europe with ten million people outside their own national territories and whose movements may promote the spread of disease is under the control of UNRRA. The new convention increases the number of diseases to be controlled, and provides for exchange of information on additional diseases. It permits additional control at frontiers, regulates the disinfection of aircraft, and imposes an obligation to keep aerodromes free of mosquitoes. In the case of important officials, certificates of of urgency may be granted to allow rapid free transit and a waiving of quarantine restrictions. For the first time complete reliance is placed on inoculation against yellow fever, an endemic disease in tropical Africa and the South American jungle, and prescribes methods of control over the manufacture and testing of the serum.

READING LIST

HORMONES AND WEEDKILLERS: W. Stiles's *An Introduction to the Principles of Plant Physiology* contains a useful account of plant hormones, which subject is also treated in H. Nicol's *Plant Growth-Substances*. The ecological aspects of the weed problem were discussed by Professor E. J. Salisbury in a lecture to the Royal Institution in April, 1942 (*Nature*, Vol. 149, p. 594) and the possibility of antibiotic mechanism having a bearing on the competition of species was raised by him in a letter to *Nature* (Vol. 153, p. 170). A standard work on auxins is Went and Thimann's *Phytohormones*.

An article, "Specific Poisons," by Dr. R. E. Slade in *Endeavour* (October 1945, p. 148) gives some particulars about methoxone.

Sister apple seedlings from the cross Doucin × Northern Spy. The one on the left is resistant to attack by the woolly aphis, whereas the other is susceptible.

Heredity *versus* Disease

C. D. DARLINGTON, D.Sc., F.R.S.

No two individuals are alike. We are quite used to that idea as applied to man. In fact, it expresses what we mean by individuals. We are less used to the idea, however, that in nature this rule holds for almost all plants and animals. As the French biologist Lamarck said, "Il n'y a réellement dans la nature que des individus." No two mice and no two mayweeds in nature are quite the same. It often happens, to be sure, that plants multiply by runners or bulbs. And when they multiply in this way the products are all the same so that one individual looks like many. But it is only in man's hands that this multiplication of individuals seriously alters the case.

When they are healthy the differences between individuals may be hard to see. But when the mouse or the mayweed is attacked by disease invisible differences come to light. One mouse or one mayweed is more resistant, another more susceptible, to the germ or mildew or insect that attacks it. And the difference is hereditary. In plants this is easily shown by the differences between standard cultivated varieties in resistance to fungus and insect pests. Sometimes this resistance is due to a specific reaction, an over-susceptibility, of the victim. And sometimes, particularly with plants that are being attacked by insects, tougher growth, such as is provided by an extra set of chromosomes, is the means of resistance.

Hereditary differences in the resistance of animals are confused by their own special method of reaction to disease and by very great differences in susceptibility to such a disease as diphtheria during development. The animal has a defence mechanism which enables it to throw off a light attack and which remains with it to repel a second attack. It acquires immunity. There are, however, hereditary differences underlying this defence mechanism itself. They are revealed by the well-known and characteristic differences in susceptibility to disease amongst different races of poultry, cattle and man. British breeds of livestock fail in different degrees to compete with native breeds in the tropics, as has now been realised at some expense. All breeds are fitted by heredity to their own country or even distinct and part of their fitness consists in resistance to disease.

In man we have an especially convenient and rigorous

way of showing the cause of these differences. There are identical twins which are really the same individual broken into two at the first cell division. Amongst these twins, even when separated in their lives, remarkable agreement is found in the incidence of disease wherever the immediate cause is universal. Thus with tuberculosis, infection is universal but a dangerous attack sporadic. Identical twins are often attacked within a few months of one another. And they are often attacked even in the same part of the same lung. Cancer is not usually due to infection at all but to a kind of internal mutation to which individuals differ in liability. As many as 36 out of 38 cases of cancer in identical twins showed symptoms in both of the twins—and often within a few months of one another. Thus the differences in resistance to tuberculosis and in liability to cancer are hereditary.

Now, wherever there are hereditary differences affecting survival, selection must alter the character of the population. We must therefore suppose that, when a great pandemic, like the plagues that ravaged Europe between 1664 and 1720, wipes out a large part of the population, not only is the surviving population less likely to be attacked by the same disease owing to direct immunisation, but also the survivors' children by selection are, on the whole, less susceptible. The whole race is altered. It is altered or *adapted*, as we say, to meet the danger it has faced and may have to face again.

Human Diseases

The human race itself, as well as crops and cattle, is continually by selection discovering new methods of defence. The results you can see very well when a species is split in two groups which neither mate nor mingle, as mankind was split before the discovery of America. Each half had its own diseases and was separately fitted to deal with them and with them only. When the two halves were rejoined their diseases were added together. Each half suffered. In his ship Columbus brought back from the New World at least one disease, syphilis, which swept Europe, Asia and Africa with a virulence which only many generations of selection for resistance have checked. In the same way, when the South Sea Islands were opened up to trade with us, the usually mild European complaint of measles ravaged those little isolated communities which had never had the opportunity of acquiring resistance by selection and killed off as many as a quarter in one year. The small isolated group suffers from new contact with a large one because it has previously been so well protected. That is one reason, and the main reason, why primitive peoples always suffer from contact with what we call civilisation.

It is on this simple Darwinian principle of the selection of the fittest that a great deal of modern plant and animal breeding has been based. Wheats have been bred with the hereditary character of resisting the rust disease. Likewise potatoes which resist blight or wart disease and apples which resist the woolly aphis. It was only when we had done all this that we ran up against a snag. Amongst the millions of rust fungi which had been living on our wheat fields were a few which knew how to attack our resistant wheat. These few had the whole field to themselves and quickly replaced their weaker brethren. Evidently the rusts were individuals also. No two of them were alike and in breeding tougher wheat we had also been breeding tougher and more effective rusts.

The same kind of warfare is going on all the time in nature. The microbe or the insect is continually by mutation and selection discovering, so to speak, new methods of attack. Bacteria themselves suffer from diseases. They are attacked by still smaller disease organisms, the viruses, or bacteriophages, discovered in 1915. In a culture of bacteria which is being attacked in this way we can get a bird's-eye view of the whole battle. One stroke of mutation by which the bacterium becomes immune to the virus is followed by a counter-stroke when the virus changes to a form capable of attacking the changed bacterium. And so the struggle goes on. Adaptation and counter-adaptation succeed one another in the laboratory culture just as in the wide world.

Man has used this device of setting a still smaller pest on to the larger. Striking victories have been won by getting a red spider to eat down the prickly pear in Australia and by collecting parasites which will destroy aphids. This weapon, however, will not meet all cases. Of more general use is the chemical killer.

Since the introduction of Bordeaux mixture to kill the blight of the grape vine, the use of insecticides, fungicides and bactericides have spread over the whole world and new types appear every year. Their use is a standard part of the cultivation of most fruit and many arable crops in advanced countries. But even this weapon is liable to break in our hands. Cyanide fumigation and arsenate spraying have been used against scale-insects in lemon groves and codling moths in apple orchards with powerful effects—at least for three or four years. But then something new happened. The parasite seemed to have become resistant to the effects of the poison. New races of parasite had been bred by the resistant survivors which no longer succumbed to the chemical attack.

Every new drug or poison, of course, sweeps all before it. Sulphonamides at first proved fatal to many bacteria, even to the hitherto unconquered *Gonococcus*. But after two years' success new races of *Gonococcus* appeared which resisted the drugs and left us as badly off as ever. A little while ago a compound was invented which killed a mould damaging fabric. Its successful application for a couple of years resulted in the appearance of a new mould which lived on the compound that was meant to destroy it. Again, penicillin has marvellous effects on innumerable enemies of mankind. But those bacteria which are not entirely killed by penicillin leave survivors whose progeny resist the drug and multiply in spite of it. When the bacterial world has recovered from the shock of penicillin it will fight back. We must be prepared for a set-back.

How are we to cope with this situation? In man and animals we already know one method of dealing with bacteria and viruses, the method of prophylactic vaccination or inoculation invented by the Turks, improved by Jenner and enormously developed by Pasteur. The method depends on preparing the defence before the attack is made. The potential victim is given a model attack which enables his blood to manufacture "antibodies" which will put the invader—virus or bacterium—out of action before it has gained a foothold. The strategy works well where the invader is constant. But if the invader is variable his

variation enables him to get through with a part of his forces intact. This is probably the trouble with certain virus diseases such as the common cold and influenza.

Virus diseases present us with other special problems. Each virus is usually carried and distributed by a particular species of parasite; there are some even which require a particular race or variant of the tick or the bug to carry them. The strength and the danger of viruses consists in their extreme variability and adaptability (including also their spontaneous origin, by mutation or transplantation, from innocent materials) as well as in their immunity to chemical attack. Moreover, the plants they attack usually fail to form or, at least to distribute, antobodies which will resist them and throw off the infection. In recent years virus diseases of crop plants have become steadily more serious in all advanced agricultural countries. Crops such as raspberries, strawberries and potatoes which 100 years ago in England certainly suffered little if at all from virus diseases now are continually subject to them. The raspberry is being steadily wiped out in England. And potatoes of most varieties cannot safely be grown more than two years without a fresh supply of "seed" tubers from parts of Scotland or Wales where the aphid which carries the virus does not live. Similar precautions are taken over large parts of Europe.

What is the explanation of this spread? All kinds of absurd reasons, such as the modern use of artificial fertilisers, have been suggested. If we examine the spread of a virus we soon get to the root of the trouble. Most viruses are not effectively passed through the seeds. Every new seedling is free from virus. It gets infected from old plants by the insect which carries the virus. And when it is infected the virus may well be of a strain which does not spread rapidly in this particular seedling. New varieties of sugar cane or raspberry are therefore often thought to be immune to virus. But sooner or later the virus changes and finds them out and another victim falls by the wayside.

Now a remarkable change has come over agriculture in all the more advanced countries of the world during the last 100 years. With seed crops, selected and uniform varieties have been replacing heterogeneous land races. With other crops vegetative propagation has been replacing seed propagation. Cuttings, grafts and tubers give an immense distribution to one identical individual. In former ages (and still to-day in the Balkans and in India) every field of wheat contained thousands of quite different plants, every orchard contained many different trees. To-day there are fewer different kinds of wheat or apples in the whole of England than there were formerly in one field or one orchard. To-day each of these kinds lasts long and spreads far. Uniformity has replaced diversity. This transformation already general in the temperate world is now spreading to the tropics.

This is a wonderful world for the seedsman and the trader, and, at first sight, for the cultivator as well. They all like a standard article. But it is a still more wonderful world for the diseases—and, one might add, for the expert who studies them and discovers new ones every year. The disease organism, whether it is a scale insect, a rust fungus, or a virus, no longer needs by its own adaptations to fit a key to every one of a million different locks. When it fits one, it fits them all. Moreover, the virus no longer needs to infect afresh every year; it remains in the tuber of the potato over winter and is ready for work in the spring. How different is the case as I described it in nature where the plant or animal is continually selected to resist to disease, continually adapts itself, and exists in unlimited variety reproduced anew every year. Now the dice are loaded against the product of plant breeding. Perfection, multiplied, standardised, and invariable, is bound hand and foot, helpless to defend itself against the variable and resourceful attacker.

The remedy then is clear. We must not seek to select, or even produce, the perfect plant and then trust to our static defences to protect it. We must at once set about replacing it by another and yet another kind. We must restore seed production and we must restore diversity to our crops.

There are many ways of doing this. In potatoes and bananas it is being done by crossing with the wild species from which our cultivated plants were long ago derived. In sugar cane it is being done by providing a regular succession of new varieties as each old one goes down. In maize it is being done by regularly crossing two inbred strains and growing only the hybrid as a crop. In raspberries it is being done by raising each plant from hybrid seed like the maize and replacing the plant by a new seedling after only a few years instead of waiting for the inevitable disaster before doing anything. In animals it is being done by crossing locally adapted races with improved but non-adapted European stocks.

In all these practices we see a sharp change of outlook. In place of the curative methods of human treatment which are necessary for man because he cannot be experimentally bred and which have until now been thought appropriate and sufficient for crops and stocks, we are altering the crops and stocks themselves. In place of cure we have genetic prevention. In place of the older medicine we have a more profound biology. Indeed, we are now reaping the harvest, the long neglected harvest, of Darwinism. It is a kind of Darwinism that would have surprised Darwin. Not least it would have surprised him in showing that the application of his theories of survival was necessary to the survival of agriculture.

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FIG. 13.—Edmund Halley's magnetic chart of the Atlantic Ocean, published in 1701 and derived from his own observations of the magnetic declination. (*From S. Chapman and J. Bartels, "Geomagnetism," Clarendon Press, Oxford.*) Halley's charts were mentioned on p. 319 in the first half of this article.

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The Earth's Magnetism

Professor S. CHAPMAN, F.R.S.

THIS historical sketch of geomagnetic progress has now brought us from the discovery of the compass, in the eleventh century, to the latter part of the eighteenth century; and it has dealt only with the *direction* of the earth's magnetic field, the declination and the dip. The *strength* or intensity of the field was still unmeasured.

Until Newton in 1687 had formulated his laws of mechanics, and until Coulomb in 1785 had determined the law of magnetic inter-action, it was not even possible to *define* the magnetic intensity; Halley had failed in all his repeated attempts, with Newton's encouragement, to discover the law of magnetic force.

Humboldt, in a famous American journey (1799–1803), observed not only the declination and dip, but also the rate of swing of his needle at different places, because it was known by that time that this rate was proportional to the square root of the magnetic intensity, provided the needle remained constant. Thus he made relative measurements of intensity, which he expressed in terms of that at a point in Peru on the magnetic equator. Such measurements are at the mercy of changes in the needle due to slight shocks and jars, so that the needle had to be most carefully guarded; even so, its strength would decrease with age. Real measurements of the magnetic intensity of the earth's field became possible only when, in 1832, Gauss showed how to determine also the strength of the swinging needle, as expressed by what is called its magnetic moment. Since that time a complete geomagnetic survey, whether of a particular ocean, country, or of the whole world, requires measurement of declination, dip and intensity at a suitable number of places.

Some civilised countries, and their dependencies, have been thus surveyed from time to time, and so also have the more frequented ocean regions. The British Admiralty has taken an honourable part in ocean magnetic surveying, and at intervals has published isomagnetic charts giving the declination and, less frequently, the dip and intensity; the preparation of the Admiralty magnetic charts since 1917 has been the duty of the Royal Observatory at Greenwich. But the world magnetic survey has never been adequate for scientific purposes, and at times even the declination chart was faulty in some of the less frequented seas. In 1904 the Carnegie Institution of Washington set up a department for the study of geomagnetism, and by ocean surveys with non-magnetic ships, and by land expeditions, it greatly improved our knowledge and brought it up to date. The world survey, however, has fallen again into arrears since 1929, when the non-magnetic ship *Carnegie* was lost by fire. To renew the survey, the British Admiralty in the years just before the war constructed a non-magnetic vessel, the *Research*, but owing to the war its work has not yet begun.

At best, however, ocean magnetic surveying is a slow job, and land magnetic surveying in the more remote and difficult regions can be even worse. Fortunately war developments have opened up the promise of a brighter future for geomagnetic science, through world magnetic surveying by air. The flight of the *Aries* was perhaps only a foretaste of what may become in time a far speedier and more accurate method of magnetic survey, so that a few years' work, to be repeated at intervals, may cover the globe with a completeness never yet approached. Technical, financial and political obstacles remain to be tackled, but there seems good hope of success.

Of the earth's magnetic past, however, we may say: The moving finger writes, and, having writ, moves on. Our record of this writing was at its best about 1920. Before 1830 it gives the direction only, without the intensity. Before 1580 it omits the magnetic dip. Even the declination record starts only at 1450, with the sun dial and map marks for Nuremberg. Van Bemmelen searched the naval archives of Holland, London and Paris for old declination measures, and therewith constructed isogonic charts for limited regions for the early dates 1500 and 1600, *inter alia* (Fig. 15). Is then the earth's earlier magnetic history lost beyond recall? Perhaps not.

During the centuries in which the compass was known, and believed to point truly northward, men used it not only for navigation and travel, but also in surveying and in setting out the ground plan of buildings. The cathedral at Magdeburg, for instance, shows slightly different orientations in parts built at successive dates, and the errors in the intended east direction may well indicate the declination at those dates. Antiquarian research on old buildings and old maps and mine-plans may still have much to add to geomagnetic history.

A quite different source of information about the dip, and possibly the intensity, in past centuries is afforded by old bricks and pottery. While cooling down, baked clay becomes slightly magnetised by the earth's field, and

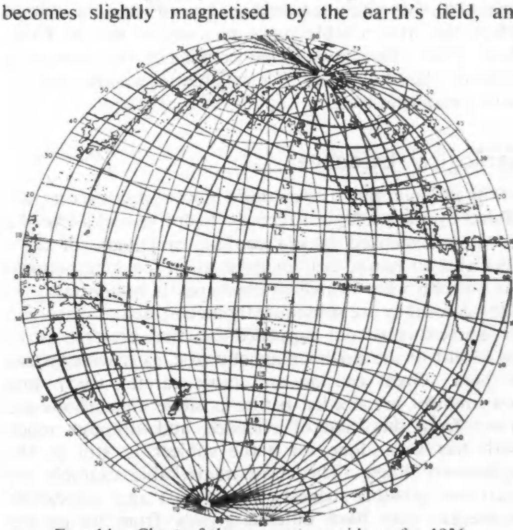

FIG. 14.—Chart of the magnetic meridians for 1836, over the Pacific hemisphere. Published by Duperrey, the chart shows the north and south magnetic poles in the positions they were believed to occupy.

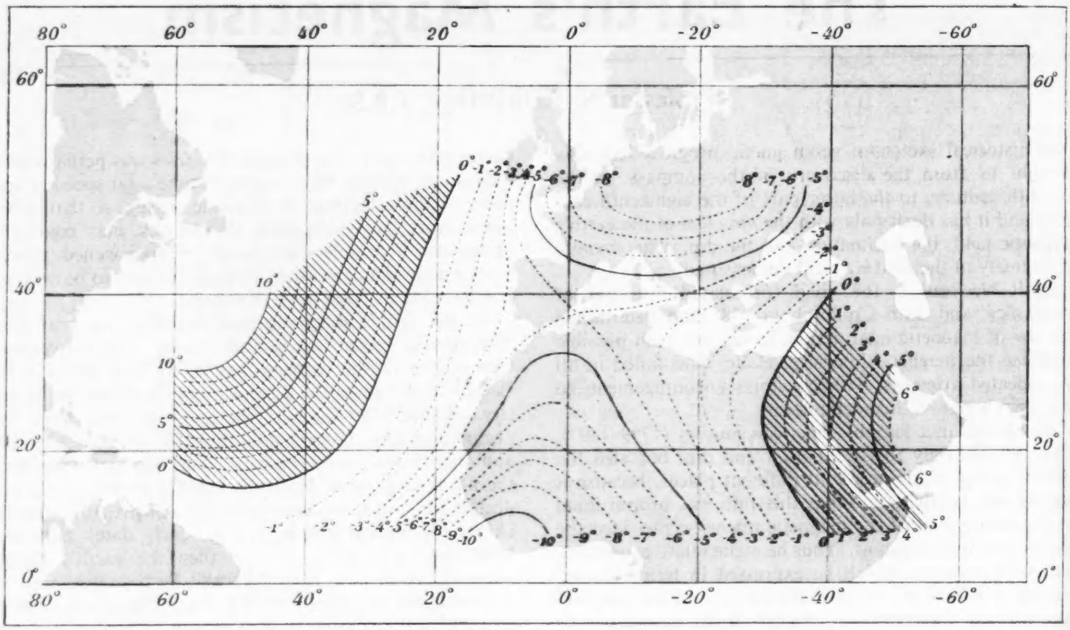

FIG. 15.—Van Bemmelen's chart of magnetic declination for the year 1500.

seems able to retain this magnetisation thereafter. A vase, for example, standing vertical while being baked, will thus preserve a record of the dip, though not of the declination. The same applies to bricks if baked in the usual position, with one of their largest faces horizontal, and used not long afterwards at known dates in buildings not too far from the kiln. In this way Thellier, using bricks in old French buildings dating from 1400 onwards, has obtained values of the dip for Paris; judging by the London observations since 1580, they give a reasonable curve of secular variation before the first reliable measurements of dip at Paris, about 1780. This gives some confidence that bricks in still older buildings may give reliable information of the earth's magnetic state.

Geological Evidence

Still grander but as yet more doubtful prospects are offered by the geological record. Eruptive rocks likewise become magnetised during cooling, and some of them seem able to retain their magnetisation for long periods, though with what degree of constancy is not certain. In many cases they are subsequently much tilted or distorted, but where this has not happened they may preserve for us the record of the earth's magnetic past, as it were, frozen in them. Great caution and most careful study and measurement are needed before positive conclusions are drawn from this kind of evidence, and although much work has been done on it the subject is still in the exploratory stage. The most remarkable example yet discovered possibly suggests that in past ages the earth's magnetism may have differed greatly from its present state, though this is not the only interpretation of the facts.

South Africa is remarkable for many evidences of ancient volcanic activity. The diamond industry centred there has its workings in what are called the throats or pipes of extinct volcanoes of a type not known in active eruption anywhere to-day; molten magma has thrust and burst its way upward through older layers of both igneous and sedimentary rocks. At first it would rise through narrow fissures and afterwards, where conditions were favourable, explosively through "chimneys". The most outstanding of these volcanic systems, the Pilansberg, is not in the diamond area, but about 80 miles north-west of Pretoria. It is a circular mountain group 18 miles wide, from which more than 14 volcanic "dykes" spread out fanwise. These dykes are nearly vertical plates or strips, 20–100 yards thick, extending upwards from great depths (several miles), in many parts almost reaching the surface though in some parts they are covered by later rocks. They are each about 100 miles long, distributed over an area about 200 miles long by 100 miles wide. They pierce through many earlier strata, and their form and coherence show that this region has suffered little subsequent distortion, unlike the Rand not far away, where the gold-bearing strata are steeply tilted. Magnetically the amazing feature of these dykes is that everywhere their magnetisation is opposite to that of the earth's field, although other geological bodies in the same region, formed both before and after the Pilansberg system, are magnetised in conformity with the earth's present-day field. These palaeozoic dykes thus appear to have preserved their anomalous magnetisation for at least 100 million years, and seem to defy any explanation other than that at the time and place of their formation the vertical field component was opposite to what it is now. It is perhaps less daring to suppose that at that time the Pilansberg system was north of the magnetic

equator, whence the continental drift has since carried it southwards, than to suppose the whole magnetic field of the earth has suffered reversal.

The magnetic exploration of this region was made with an instrument called a variometer, which measures the departure of a component of the field from its value at a base station in the region; in this case, as is most common in such local magnetic surveys, the anomalies measured were those in the vertical component. Such magnetic measurements provide a cheap, rapid and simple method of prospecting for ores of different kinds, especially for those of iron—a practice dating back at least 300 years in Sweden, where magnetic anomalies abound. In the far north of Sweden a deposit of almost pure magnetite, the strongest magnetic oxide of iron, produces a great local increase in the field intensity, up to six-fold the value normal in that region; this is partly because the ore extends to the surface. It consists of a long strip only 30–160 yards thick.

The greatest known magnetic anomaly is that of Kursk, 250 miles south of Moscow; it consists of two long narrow deposits, running parallel about 40 miles apart, and up to 150 miles long. The actual deposits are thin strips up to 400 yards thick, steeply inclined to the vertical; as they do not approach within 150 yards of the surface the observed magnetic anomaly is less intense than over the Swedish deposit. Another notable Russian magnetic anomaly lies near Krivoi Rog. Finally, a Finnish island in the Baltic may be mentioned where a deposit of magnetite doubles the earth's field, and by affecting the compass has caused many shipwrecks.

As copper and gold are usually associated with heavy magnetite, magnetic surveying is helpful in locating them indirectly. This is, of course, only one of many methods of geophysical prospecting.

Leaving these important but detailed features of the earth's magnetism, let us consider the main characteristics. Gilbert's assertion that the earth is a great magnet is fully confirmed, not only by the distribution of dip and declination, as shown on Duperrey's map (Fig. 14), but also by that of intensity. The mathematical theory of magnetism, developed since about 1800, shows that on a uniformly magnetised sphere the intensity should increase steadily from the magnetic equator to the poles, where it should have twice the equatorial value; observation confirms this, though with important regional anomalies. The axis of the uniform magnetisation that best fits the whole distribution is inclined at about 11 deg. to the geographical axis; the north end or pole of the magnetic axis is about 700 miles from the actual north magnetic pole (Fig. 16).

Gauss showed how by a mathematical analysis of the distribution of the north, west and vertical components of the field it was possible to separate the field into three parts—one originating within the earth, one with its source above the earth, and one due to electric currents crossing the earth's surface. The analysis of the available observations shows that about 95 per cent is of internal origin, thus substantially confirming Gilbert's inference from his simple dip experiments. The other two parts, each amounting to 2 or 3 per cent of the whole field, are ill-determined and still uncertain; their magnitude cannot be established until the world magnetic survey is made more accurate and more complete.

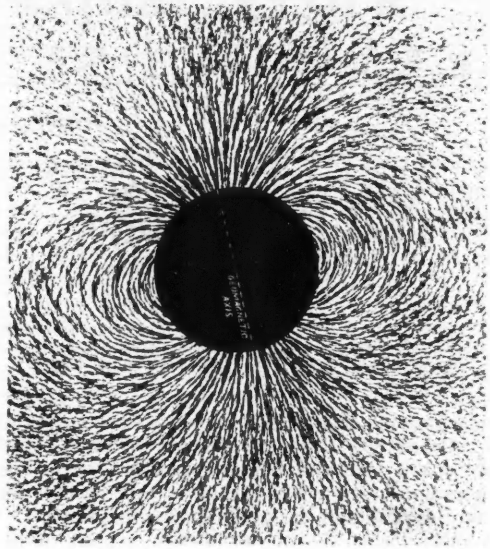

FIG. 16.—Diagram showing the lines of magnetic force for a uniformly magnetised sphere.

Subject to these minor uncertainties, it is possible to calculate how the earth's field should be distributed in the space round the earth; with increasing height or distance from the surface, the influence of the regional irregularities decreases and the field approximates ever more closely to that of a simple spherical magnet. The advance of radio science has made it possible to measure indirectly the magnetic intensity in the ionosphere, where Sir Edward Appleton has confirmed the expected decrease, at that height, of about 10 per cent. Clear evidence of the far greater but ever weaker extension of the field beyond the earth is afforded by the distribution of the northern lights and of cosmic rays. The electrically charged particles associated with these, coming from the sun or from still greater distances, are deflected polewards by the magnetic field, which profoundly modifies their incidence upon the earth.

Various complex and interesting phenomena linked with the sun, the northern lights, and with tidal ebb and flow in the ionosphere produce small transient disturbances and small periodic changes of the earth's field. These are of great scientific interest, and of practical importance in connection with cable and wireless telegraphy. Much progress towards their understanding has been made.

But the main fact that the earth is a great magnet remains an unsolved problem, a standing challenge to theoretical physics. Kelvin and others have expressed the conviction that the cause must be associated with the earth's rotation, and many theories based on this idea have been devised, but none of them seems satisfactory. At best, any purely rotational theory will leave unexplained the oblique part of the field, which is about 10 per cent of the whole.

The simplest theory, that the bulk of the earth is magnetised, meets the difficulty that only a thin outer layer, which itself is not adequately magnetised to account

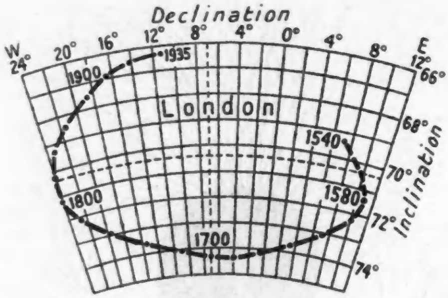

FIG. 17.—The variation of magnetic declination and dip at London since 1580 (and conjecturally from 1540 to 1580).

for the observed field, is cool enough to retain magnetisation. Conceivably the great pressures within the earth might enable matter to remain magnetised at the high temperatures within the earth, but the experiments made to test this possibility do not favour the suggestion.

Another theory is that the earth is a great electromagnet, but we have no understanding of how the necessary electric currents could be either created or maintained. Otherwise, even if they are present, they must be decaying owing to the earth's electrical resistance, of which something is known. The calculated rate of decay would imply an immensely greater earth magnetic intensity in the past, of which geology gives no indication.

It is, indeed, of great interest that the general intensity, or magnetic moment, of the earth's magnetism appears to have decreased by about 5 per cent in the last century; but it would be unsafe to conclude that this is a regular decrease which has long continued and will persist.

There seems to be a widespread impression that the secular change of the earth's magnetism is regular, and consists of a regular motion of the magnetic poles round the geographical poles. Actually the data give little support to this rotation, and the secular change certainly cannot be predicted. It is, indeed, less simple than the main field—a regional, not a planetary phenomenon. Its cause is unknown, but it seems almost certain that it is produced by internal earth movements of which we have no other clue.

Our longest series of measures of the magnetic direction is that for London from 1580 onwards. The curve that during these three and a half centuries the needle at London would have traced on a sphere shows three-quarters of a roughly oval path, with a range of 8 deg. in dip, and 35 deg. in declination (Fig.17). It is pure speculation to suppose that the oval will be completed in a century or so, and thereafter repeat itself. If the change were periodic its period would be about five centuries, but the changes at other places suggest that there is no one period applying to the whole earth, and perhaps none even at any one place. The key to the problem of the great earth magnet, and of its changes, may perhaps be within our reach, though as yet unfound. It may, on the contrary, be beyond the present resources of theoretical physics, dependent on future advances, and perhaps also on further long continued observation of the magnetic changes.

(This article is the substance of a lecture given to the Royal Society of Edinburgh in July, 1945.)

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(1) *The Earth's Magnetism* (Methuen's Monographs on Physics).
(2) *Geomagnetism* (2 vols., Oxford University Press), where many further references are given.

Sefstrom's Centenary

IN contrast with Scheele and Berzelius, a disciple of the latter named Nils Gabriel Sefström has been almost completely neglected in the history of chemistry. Yet Sefström, who died a century ago on November 30, should be remembered not as one-time professor at the Institute of Medicine and the School of Mines, Stockholm, but for his discovery of vanadium in some iron smelted from the Swedish mine at Taberg. Vanadium had a remarkable history before those days when the search for supplies drove the brother metallurgists Flannery to scour the world, eventually discovering rich supplies of patronite high up on Andes, and bringing it down on the backs of llamas. The Mexican professor Del Rio had been on the track of the unknown element when he referred to "erythronium", a body-forming rose-coloured salts and found in some brown lead from Zimapán.

Independently Sefström, becoming curious when a black powder left from dissolving iron in hydrochloric acid contained not only silicon but something new, set to work although only a few decigrams were available. He was delayed when extracting the new base from a slag, for some students spilled the vanadium solution and Sefström had to repeat the whole laborious extraction. But although neither he nor his master Berzelius isolated the free metal, full credit is due to him for his paper on the properties of vanadium delivered before the Academy of Science. How the eminent Wöhler must have kicked himself for missing vanadium in the Zimapán brown lead after Del Rio's days! "I am an ass," he cried, and was just as chagrined as was the great Liebig when Balard beat him in the race to discover bromine (Liebig put a sample in his "cupboard of mistakes"). The master Berzelius must have been bemused, for in romantic vein he told how in the far north lived the goddess Vanadis; how Wöhler first knocked timidly at her door, but went away; and how Sefström came and knocked repeatedly and was admitted.

Sefström must also be allowed a place in the history of platinum fabrication. For with his orginal eight-blast portable forge with its conical tuyères from the bellows he melted platinum, while Berzelius made his platinum crucibles alloyed with silver and free from bubbles.

Equal Pay for Equal Work

AN ECONOMIST LOOKS AT THE PROBLEM

L. DELGADO, Ph.D.

THE large numbers of women employed in this and other countries during the war, and the need that we shall have to avail ourselves of this labour in the future, have brought to the fore a problem forming part of the larger question of wages—why women earn lower wages than men. Both in and out of Parliament this lesser question has been the subject of much discussion and study. In many cases the discussion has been detached from the practical sphere—examining the general principle governing the payment of wages—and has wandered into the sentimental, into which we, as scientists, cannot enter.

In order to discover why women do not obtain equal rates of pay as men for equal work it behoves us to examine the larger question of wages in general. Now, economists have long been at loggerheads on the matter, and it is only recently that a scientific explanation has been forthcoming. It is undoubtedly true to say that human labour ought not to be treated as a commodity to be bought and sold on the open market, but it is a lamentable fact that to a large extent it is still so treated.

But we are here concerned not with what *ought to be* but with *what is*. We must realise that the entrepreneur looks upon labour, male and female, in the same way as he considers other resources of production, i.e. in an impersonal manner—simply as a means to an end. Various explanations have been given in the past of the principle involved in calculating the proportion of the product that goes to labour. We had, for instance, the Susbistence Theory of Wages, which undoubtedly applied throughout long periods in the past and may probably still be true in some of the thickly populated agricultural countries of the East. The basic idea behind this theory is that the force of numbers pressed down wages to bare subsistence level; beyond this earnings could not fall as starvation reduced the number of workers and wages rose until the increase in the population that this encouraged again dragged down the standard. But the theory does not fit in with the facts; in the last seventy years of the nineteenth century, although the population grew steadily, wages were trebled.*

The favourite theory propounded during the period of the Industrial Revolution in England was the Wage-Fund Theory. It was thought that, since the worker must live during the time taken in production, the payment must come out of the accumulated stores of past production, i.e. by an advance from capital. J. S. Mill believed that wages depended upon the proportion between population and capital. From this it followed that so long as the ratio of the total population to the total capital remained constant, wages could rise in one industry only at the expense of another. Here again, facts refuted the theory. In any case the total number of workers is not the same as the total population, nor is the amount of capital advanced as wages the same as the whole capital of industry. The idea underlying this theory has since been modified, but it contains two important germs of truth. Firstly, it emphasises the part played by the relative scarcity of labour in determining wages; and secondly, that the wages of labour come from the product of labour. The modern marginal productivity theory of wages flows directly from the wage-fund theory.

It is perfectly obvious that labour commands a wage because it helps in the production of something which has a money value. Entrepreneurs are willing to pay for this service, and the greater the service rendered the more they are willing to pay—hence the productivity theory. Theoretically, an employer will limit the wages of a particular worker to the amount he is worth to him. The rigidity of the labour market to-day, as a result of the organisation of labout—apart from considerations of convenience—obliges us to broaden this principle from its application to a single individual to include a general class. The problem is complicated by the fact that the national dividend does not all go to labour: it is distributed also as rent and interest (the rewards of land and capital respectively), and the factors that take these rewards are continually varying in amount and in proportion to one another. Moreover, the entrepreneur himself is entitled to a share—which is not fixed—of the product unappropriated by land and capital (in which are included all the factors not classed as labour). It is a fact that if wages fall below the productivity of labour the competition of employers will force up wages, while if wages are higher, competition will bring them down by lowering the marginal product. Under conditions of perfect competition (i.e. in the absence of conditions making for rigidity), the wage will be such as will leave no surplus of unemployed, for if there were any they would offer themselves at a lower rate. To prevent themselves from being displaced, the labourers in employment would have to accept lower wages. The value of the marginal product of labour would thus be greater than the new wage, and the producers would therefore increase their surplus of labour until marginal productivity was equal to wages.

In a modern industrial system it is not practical to fix wages individually, while even under conditions of perfect competition a standard rate would for convenience be paid, which would on the average be equal to the productivity of the workers. This is, in fact, the principle adopted and it means that the best workers are paid less than the value of their product and the worst workers get more than their share. If the best workers object, the employer may be forced to raise their wages to the level of the marginal product, and if the employer does so he must give the worse workers lower wages or dismiss them. If one firm pays higher wages than another, workers will be attracted from outside, and the marginal product of labour in that firm will fall while the product in other firms will rise. Thus wages throughout the area, other things remaining unchanged, will be the same for identical work.

It is true, of course, that the relative scarcity of labour is an important factor in deciding the amount of wages.

* It need hardly be added that throughout this article the term "wages" means *real wages*, i.e. wages expressed in terms of purchasing power, and not nominal wages, which are wages in terms of currency.

This is not inconsistent with the marginal productivity theory. The idea of a margin implies relative scarcity. The demand for labour is derived from the demand for its product.

How does all this affect women? Women labour under special disabilities which render them, on the average, less efficient than men. The marginal product of their work is therefore less than that of men and their remuneration is correspondingly smaller.

In the first place, woman is the mother of mankind, and this function must normally shorten the time during which she can be at work, at a time, too, when she is in the full vigour of her life; her children will demand a certain minimum of care and attention which cannot prudently be delegated. Hence her working day is shorter than a man's. The provision of crèches in factories and elsewhere has gone some way to prevent absenteeism due to this cause, but it has not entirely solved the problem. Moreover, these crèches cost money to install and to staff, a cost which must be met out of the productivity of labour. It cannot be met at the expense of labour in general, for entrepreneurs that employed men only would be in a favoured position and would attract the best workers.

It is obvious that for certain occupations, notably in the heavy industries, women have not the physical strength to do the work required. Present-day technical improvements tend to replace brute strength by skill: nevertheless, we expect to find these industries in the hands of men, if only from the fact that the skill is acquired over years of practice.

Another important factor is that for a man, commerce or industry is a permanent vocation. In normal times most women leave their work when they marry: they have, in fact, no intention of making a success of their job and their efficiency suffers. Any skill a woman might have acquired is thus lost to society.

Now, all these are excellent and sufficient reasons why women are paid less than men for similar work. There are other reasons which are thoroughly bad and unjust. One of them is the force of custom. If a woman can, over a long period, do a job as efficiently as a man, justice demands that she should be paid the same wage, no matter what custom and privilege have held in the past. Another reason is that women themselves are willing to accept considerably less than a man in order to obtain employment, especially when they are not dependent upon their wages for their livelihood. For this reason women, even to-day, tend to crowd into the drudgery and worse-paid occupations, where the force of numbers helps to keep down their wages. This is merely one aspect of the productivity theory: the overcrowded labour market has forced down marginal productivity and, with it, wages. Again, women are less inclined than men to enter militant organisations for the purpose of forcing up wages. I believe, however, that though wages might be forced up in this manner they would not be raised to man's level, whose labour yields a greater return.

Women in Science

Let us now leave the field of general inquiry, and look more closely into an industry that interests personally many readers of this journal. There seems no doubt from the most cursory examination of advertisements offering posts in the scientific world that employers like to take advantage of the cheaper labour of women, and that women look upon this as natural. Why should this be so? What are the forces that appear to operate so powerfully against the general principle that wages depend upon the productivity of labour? Briefly these are as follows.

The first factor is the force of numbers. With the spread of education scientific work has appealed more and more to women. A girl who is clever at science at school fits easily into this extension of her interest: it is work that she likes and one that gives her a certain amount of prestige. This question of prestige is not one that can be lightly discounted: it forms an intangible addition to wages and has long had a powerful effect on the remuneration of men in certain occupations, particularly in public office. Scientific work appeals to women—and to their parents—because usually the environment is of a high intellectual order.

The second factor is that women are comparatively newcomers in the field of science and have not yet found their level in the industry. We do not yet know the exact value of women's work in science. We could all name twenty famous men in the world of pure science, but we should all be hard pressed to add the name of even one universally known woman to that of Mme. Curie. Moreover, modern scientific work is so much the product of teamwork that it is difficult to measure the contribution of each member. Even the final result is difficult of measurement. And any research worker may toil for years, and the final result elude him.

Another factor is not unconnected with this. Women rarely enter scientific work with the object of following up any particular branch of work to its conclusion. Usually they leave when an opportunity for marriage arises. This means that the value of the continuity of past work is largely destroyed and that women rarely achieve that perfection in work which is the result of long practice. This must be an important consideration in fixing the wages of women in this field.

Scientific Work Incidental

On the whole, women go in for scientific work simply as an incident in their lives, even if they have no intention of marrying. A woman who has had some scientific training at school is usually the daughter of parents in comfortable circumstances or at least they are partly supported by them, and in so far as this is true the question of salary is not of pressing importance. For this reason, women do not organise themselves for improving their conditions of work. Even if this class is in a minority in these days, it is a minority that competes for employment, and its competition tends to force down wages.

In many occupations, especially in those in which the product is difficult to measure, wages are determined more or less arbitrarily, and a lower wage to women has been justified on the grounds that man has greater social responsibilities. It must be admitted that to an economist this explanation is as unscientific and as unsound as that explaining lower wages for women as due to the force of custom. Both are thoroughly bad and are to be condemned.

[Continued on p. 344

Fellmongering Research

AUSTRALIAN SCIENTISTS STUDY SEPARATION OF WOOL FROM PELTS

W. J. ELLIS, F. G. LENNOX and MARGARET E. MAXWELL

THE importance of the sheep industry in Australia needs no emphasis. The associated industry of fellmongering, however, which is concerned with the removal of wool from sheepskins, deserves more attention than it normally receives. Over 20 million skins were produced annually in Australia prior to the war and, since then, largely owing to the increased demand for lamb and mutton by the allied fighting forces, the number has increased. Before 1939, slightly less than half the skins were fellmongered locally. The remainder were exported, most of them to Mazamet in France, where cheap labour, proximity to markets for wool and pelts, a plentiful supply of fresh water, and an equable climate provide good conditions for the industry. The fall of France threw an additional burden on Australia. The situation was met by increasing fellmongering capacity, by tanning some skins with the wool on to make floor coverings, linings for airmen's flying suits and so on, and by drying the remainder and storing them until such time as they could be used locally or exported. Thus, the need for using highly efficient methods of fellmongering in Australia is probably greater now than ever before. Maximum efficiency will only be attained through the application of scientific knowledge to the industry, and it is with this object that the Australian Council for Scientific and Industrial Research* initiated fellmongering research a few years ago in the Bio-chemistry Section of the newly formed Division of Industrial Chemistry.

Two methods of fellmongering are employed in Australia. One is the sweating method, which depends on the natural loosening of the wool due mainly to bacterial action, occurring when skins are kept moist and hung in a closed chamber. The other is the painting process, which depends on the digestion of the wool roots by a lime sulphide depilatory paint, applied by brushing it on the flesh side of the skin. In most countries sweating has been entirely superseded by painting, but in Australia the earlier process is still used extensively for fellmongering Merino skins. It is difficult to control the bacterial development in the skins during sweating; damage to the skin tissues is likely to occur, but the wool is recovered in excellent condition. In contrast to sweating, the painting process involves no damage to the skin tissues, but it is liable to harshen the wool. Thus sweating is preferred for Merino skins, while painting is preferred for skins with coarser wool but better pelts. Since Merino sheep are reared in the northern and eastern grazing areas in Australia, most of the fellmongers in Brisbane and Sydney use the sweating method. The plainer-bodied English breeds and cross-bred sheep are run in the cooler region to the south, and consequently many of the skins treated in Melbourne and Adelaide are painted. In New Zealand, where the sheep are mostly plain-bodied, the painting method is used exclusively.

The operations employed at the sweating fellmongery up to the stage of wool loosening resemble those used in the painting fellmongery. The skins are washed and soaked in water to remove blood, dirt and suint and, if they have been preserved by drying, to restore the moisture content of the tissues close to that of fresh sheepskin. Owing to the prevalence of burrs in Australian pastures, it is usual to pass the skins through a "deburring machine" after soaking. Here rapidly rotating rollers with knives attached and jets of water dislodge foreign material from

*Australia's equivalent of Britain's Department of Scientific and Industrial Research.

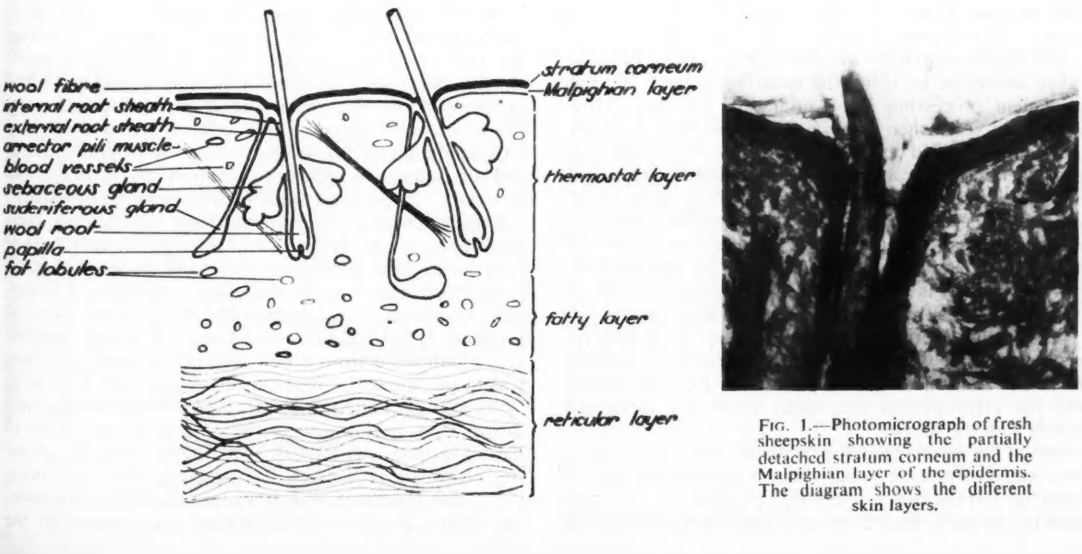


FIG. 1.—Photomicrograph of fresh sheepskin showing the partially detached stratum corneum and the Malpighian layer of the epidermis. The diagram shows the different skin layers.

FIG. 2.—Cross section of fresh skeepshin showing wool root surrounded by cells of the sheath and other structures.

FIG. 3—Cross section of fresh sheepskin showing collapse of tissues during the withdrawal of a wool fibre.

the wool. Sweating is then allowed to proceed by hanging the moist skins in closed chambers for several days until the wool has been sufficiently loosened for easy pulling. This is performed by "pullers" who spread each skin over a convex sloping beam and remove the wool by pushing it down the slope. Attemps to substitute mechanical methods for manual pulling have been unsuccessful, mainly because the wool is roughly classed into several lines as it is pulled, and this classing depends on the judgment of the puller (Fig. 5).

The above treatments are invariably applied in Australian fellmongeries using the sweating method, but the subsequent processing is by no means uniform. Most fellmongers scour their own wool, but the pelts are either dried and exported as "slats", washed and limed and sold to neighbouring tanners, or delimed, bated and pickled for export. Some fellmongeries are run in conjunction with a tannery where the pelts are vegetable-tanned to produce basils, converted to fancy leathers, or oil-tanned to produce chamois leather. The light leather obtained from sheepskins is suitable for making such articles as handbags, gloves, hatbands, shoe-linings and aprons.

When fellmongering investigations were being planned, it appeared that the sweating process was more urgently in need of assistance than the painting process, and therefore the investigations mentioned below are concerned mainly with the former.

One of the major difficulties experienced by fellmongers who use the sweating process has been that the rate of sweating is at the mercy of climatic conditions. Not only does the process require up to 8 days in the winter, as compared with 2-3 days in midsummer, but day to day fluctuations in temperature frequently cause sweating to be finished when labour is not available for pulling the wool. This may result in over-sweating and serious damage to the pelts. Yet another urgent requirement is a better method of recovering the wool from skin pieces, heads and shanks than those at present used. The usual method of treatment is to spread the pieces in a layer about 6 inches thick on the floor of the fellmongery and allow the tissues to putrefy well beyond the stage of sweating. The wool is easily recovered from such material by hand-picking, but it is damaged by the heat, ammonia and other products of putrefaction produced during the treatment. Moreover, such objectionable odours are evolved that picking the pieces for recovery of the wool is distasteful and it is sometimes difficult to obtain labour to undertake this work. These are typical of fellmongering problems.

Some interesting and pertinent facts have already been established in laboratory experiments. It has been shown, for example, that 75 different strains and species of bacteria occur fairly regularly on sheepskins. Samples of sheepskin have already been obtained from various parts of England, U.S.A., Canada and Eire, and further samples will be obtained from other countries to determine how widely the species of bacteria recovered from Australian skins are distributed and, therefore, whether deductions drawn from experiments with local skins are likely to have application elsewhere. To determine which of the organisms are responsible for loosening the wool during the sweating process, sheepskin was chemically sterilised, the sterilising agent was destroyed, and samples of the

prepared skin were inoculated with each of the bacteria in turn. When tested in this way, only four organisms were found capable of loosening the wool, and of these four, one, a strain of *Proteus vulgaris*, normally preponderates on the wool roots at the completion of sweating under natural conditions. Similar results were obtained by inoculating sterile foetal lambskin instead of sterilised adult sheepskin. The foetal skin was dissected off embryos obtained from almost full-term pregnant ewes killed at the abattoirs. Partial loosening of the wool was observed on both adult skin and on foetal lambskin without the intervention of bacteria, but for complete loosening it was necessary for the bacteria to penetrate to the base of the wool root and partially digest it. This occurs normally during sweating. The wool roots become tapered and offer little resistance to removal of the wool by pulling. It was hoped that heavy inoculation of sheepskins with the principal sweating bacterium would produce a considerable increase in the rate of sweating, but actually the sweating time at 25° C was only reduced from 38 hours to 30 hours. Washings of sweated pelts, which would be heavily charged with this organism, produced a similar effect. It is doubtful, however, whether the small saving of time would justify the cost of this treatment.

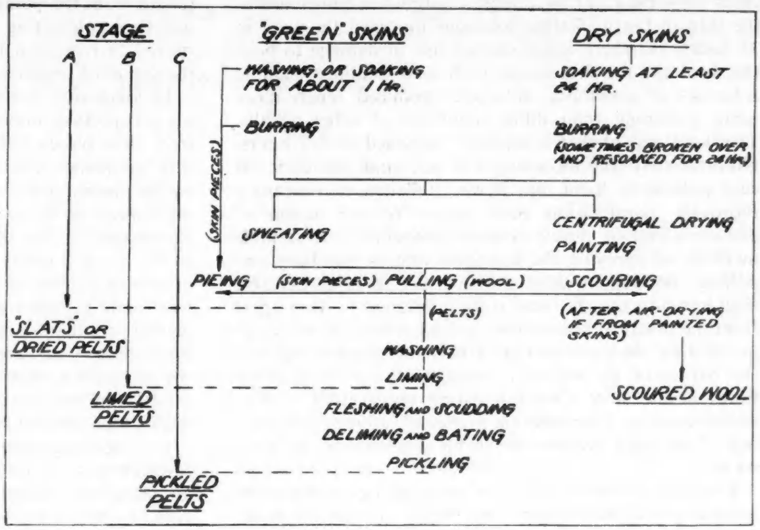


FIG. 5.—Flowsheet showing the treatments employed in Australian fellmongering and the products obtained.

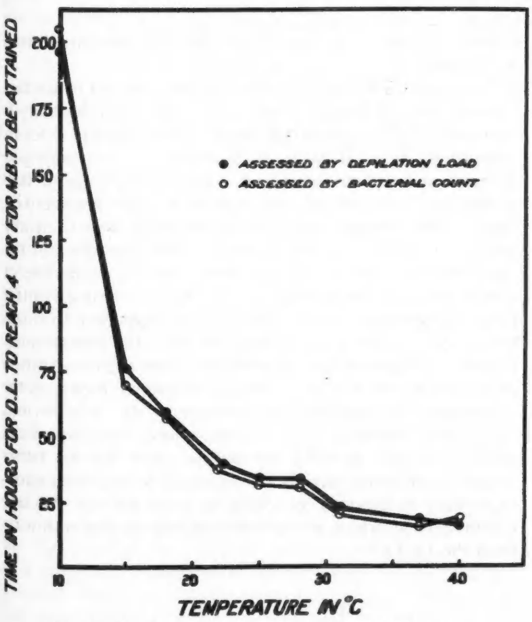


FIG. 4—Results which demonstrate the important effect of temperature on the rate of sweating. DL means "depilation load"; MB means "maximum bacterial growth."

It has already been stated that the time of sweating varies with the temperature and it was therefore desirable to examine this relationship fairly closely. By sweating samples of skin at various temperatures, a close similarity was revealed between the curve relating temperature with time for completion of sweating and temperature-growth curves for bacteria. Indeed, there is a very rapid increase in the rate of multiplication of *Proteus vulgaris* just prior to completion of sweating. Raising the sweating temperature from 10° C to 28° C produced a marked increase in the rate of sweating, but further increase in temperature had little effect (Fig. 4).

Relative humidity also affects sweating. All evidence obtained so far suggests that it should be maintained as close as possible to saturation. If it falls, moisture evaporates from the skins and they become too dry for the sweating bacteria to develop. Evaporation of moisture also cools the skins and thereby retards sweating.

As a result of studies on the effect of temperature and humidity, it has been possible to offer advice to the industry concerning the construction and control of sweat-houses. It is understood that the results obtained in houses erected in accordance with this advice have been such that the extra installation costs will be speedily recouped.

Studies of the soaking operation have shown that one of the initial effects of water penetration into the skin tissues is to tighten the wool slightly, but fortunately this effect is only temporary. No advantage could be demonstrated for long soaking and, if held continuously in water instead of sweating in the air in the usual way, wool loosening was greatly retarded. Slight variation in the initial acidity or alkalinity of the soak water had little

effect on subsequent sweating. Continuous immersion of the skin in fairly alkaline solutions loosened the wool in 24 hours, but there was a serious risk of damage to both the wool and the skin tissues with such solutions. Dilute solutions of ammonia, however, produced much more rapid loosening than dilute solutions of other alkalis. The wool was almost completely loosened in 3-5 hours. Unfortunately, the loosening was not quite sufficient for easy pulling by hand, but it was sufficient to warrant a thorough investigation being made of the action of ammonia and of closely related compounds. If suitable methods of applying the ammonia and of handling and pulling the treated skins are evolved, it is conceivable that ammonia may be used in the fellmongery. But, apart from the practical possibilities, a study of its action is well justified on the grounds that it may throw some light on the nature of the materials responsible for holding the fibres in the skin. One interesting observation is that, unlike existing fellmongering processes, ammonia loosening of the wool involves no visible digestion of the wool roots.

Frequent reference has been made above to the wool-loosening and wool-tightening effects of various treatments, and little progress could have been made in research on the industry without a suitable method of assessing the ease of pulling wool from the skins. The method devised involves measuring the pull required to remove a bundle of wool fibres from the skin, cutting a standard length from the bundle, scouring and weighing the fibres, and then calculating the pull which would have been required to remove a bundle of fibres which, when cut to the standard length and scoured, weigh 1 milligram.

In conclusion, it is perhaps worth mentioning that there are prospects of improving the method of recovering wool from skin pieces. The unpleasant methods employed in this particular fellmongering process may be improved by the application of scientific method. The most promising appears to be by the use of protein-splitting enzymes. Shrinkage of the skin collagen by heating the pieces to 65° C for 2 hours, and incubation in a watery extract of a bran culture of a mould containing such an enzyme was found to digest the skin tissues without any apparent damage to the wool. Possibilities of using this process on a large scale are now under investigation. Papain, the proteolytic enzyme contained in the milky latex obtained from the skin of green papaw fruits, is also effective, but its high cost is likely to preclude commercial adoption.

It is encouraging to note that, although the fellmongering research was started by the C.S.I.R. purely as a service to an important industry, the industry itself has recently come to realise the value of the research being done and is making a substantial contribution towards the cost of the work.

(For futher information on this work readers are advised to consult the C.S.I.R. Bulletin No. 184, *Fellmongering Investigations*, published Melbourne, 1945.)

EQUAL PAY FOR EQUAL WORK—*continued from p. 340*

The factors that we have discussed are of varying merit and explain only in part why women are paid at a lower rate. To be perfectly just it must be admitted that employers take an unfair advantage of these factors to press down wages below the amount to which productivity entitles women.

A Task for I.L.O.

In discussing these points, we have chosen a difficult industry, one in which the special forces we have examined appear to modify seriously, and even to contradict, the general principle enunciated earlier. Scientists will readily understand that an underlying truth may be modified by adventitious circumstances. But there is nothing in the facts that cannot be explained by the theory of marginal productivity. However, the fact remains that it is bad for labour, male or female, to be rewarded below the amount to which its productivity entitles it. It is true that it is a policy that yields immediate results, but it is one which is disastrous in the long run—disastrous not only to labour but to the industry as a whole, if only because of its effect upon efficiency (and there is much more in it than this). For this reason alone the work of the International Labour Office is to be encouraged, even among the intellectual occupations.

One word of warning: in the industrial world of to-day, where profit is the mainspring of enterprise, productivity is measured in terms of market value and not merely in terms of output. Thus all sorts of accidental factors may give different rewards to men and women of the same skill doing equal amounts of work, and though the productivity theory is eminently more satisfactory to labour than any other, it may yet lead to injustices. But here we enter the field of ethics: it is in these realms—on the borderline of ethics and economics—that the I.L.O. will find a fruitful field of operations. This organisation may also do much to correct the manifest imperfection in the productivity theory arising from the circumstance that the competition of employers for workers is by no means so intense as the competition of workers for employment. It is not obvious that human wants and desires are a long, long way from being satisfied? Is there not ample scope for the better organisation of our economic resources so that unemployment may be reduced to negligible proportions? No one can take the lead here with greater knowledge and authority than the I.L.O.

Is F.M. Broadcasting Coming to Britain?

D. A. BELL, M.A., A.M.I.E.E.

THE initials "F.M." stand for "Frequency Modulation", and, although this may not mean much to the man in the street in Britain, F.M. is a big event in the United States. It has been headline news in the technical press since 1940, has performed valuable service in the U.S. Army communications systems, has been a storm-centre of the radio industry in 1944-5 because it was regarded as a serious rival to television, has introduced high-quality radio reception to the public, and has been regarded by the C.I.O. (Committee for Industrial Organisation) as a possible means of breaking the big-business monopoly of the broadcasting networks operating on the older system of transmission ("A.M." or "Amplitude Modulation" system) which is used by all medium- and long-wave stations.

The great merit of F.M. is that it is easier to separate the wanted programme from all kinds of interference than is the case with A.M. transmissions, and the two results of this are the encouragement of higher quality of reception and the possibility of having numbers of transmitters serving limited areas without mutual interference. The localisation of the service area of F.M. transmitters also arises partly from the use of ultra-high frequencies (better known to British listeners as ultra-short waves), the reason for which will become clear from the technical description of F.M.

In approaching the technical aspects of F.M., the first point to examine is the general relationship between modulation frequencies and radio or carrier frequencies in radiotelephony. The range of frequencies of audible sound is approximately from 30 cycles per second to 15,000 c/s., and a microphone can be made to generate an electric current having the same frequency or frequencies as the sound waves impinging on it and having an intensity proportional to the intensity of the sound; such a current can be transmitted by wire, with amplification if necessary, and used to operate at the far end an electro-acoustic instrument, e.g. a telephone ear-piece or loudspeaker, which reproduces sound corresponding to the varying electric current. Now the essential feature of *radio*-telephony is that instead of being carried by wire from transmitter to receiver, an electric current at the transmitter is caused to generate electromagnetic waves which are propagated over great distances and are able to set up corresponding electric currents in the receiving aerial; but an electric current varying at audible frequencies (less than 15,000 c/s.) is not very effective for generating electromagnetic waves, and it is therefore necessary to employ a high-frequency current, usually called a radio-frequency current, to generate the electromagnetic waves, and to vary or *modulate* this radio-frequency current in accordance with the audio-frequency currents generated by the microphone. Radiotelephony therefore involves two stages of conversion at both transmitter and receiver between sound and audio-frequency electric current and between audio-frequency current and modulated radio-frequency current. The radio waves which travel from transmitter to receiver have the same frequency as the radio-frequency electric current which generated them; this frequency is known as the "carrier frequency" and the propagation characteristics of the wave—attenuation in various parts of the atmosphere, reflection by the ionosphere, diffraction, etc.—vary with this frequency. Units for radio frequencies are kc/s. (kilocycles, or thousands of cycles, per second) and Mc/s. (megacycles per second), and the carrier frequencies which have been used range about 16 kc/s. to an upper limit which is probably still a military secret but is certainly not less than 3,000 Mc/s. (3,000,000,000 cycles per second).

Since the transmission of a steady radio-frequency wave cannot convey any information, it is necessary to modulate the carrier wave with the programme to be transmitted, and the modulation may be a variation in the amplitude, frequency or phase of the carrier wave. Expressed mathematically, the carrier wave is of the form $E = A \sin(2\pi ft + b)$ and any one of A, f and b may be modulated by the audio frequencies to be transmitted. It must next be stated as a general proposition that modulation of any one of these quantities by a frequency n will produce at least the frequencies $f+n$ and $f-n$, so that transmission of the full range of audible frequencies involves not only a single carrier frequency but also a band of frequencies extending at least 15 kc/s. on either side of the carrier frequency; these additional frequencies on either side of the carrier are known as sidebands. With amplitude modulation there is just one pair of sidebands for each component frequency in the modulating signal: for example, a carrier of frequency 41,000 kc/s. modulated by a single note of 7 kc/s. would be resolved into the three frequencies—40,993 kc/s., 41,000 kc/s. and 41,007 kc/s. But with frequency modulation there would theoretically be an infinite series of new frequencies; in our example they would be spaced at intervals of 7 kc/s., and they would have amplitudes which are expressed in terms of Bessel functions.

However, the case which interests us in practice is the condition that the change of frequency employed as the maximum depth of frequency modulation is considerably greater than the highest audio frequency in the modulation; this is true of the present standard in U.S.A., having a frequency variation of $\pm$75 kc/s. for full modulation, which is five times the maximum audible frequency. Under this condition, we shall not be far out in assuming that although the series of sidebands is nominally infinite the only ones which have sufficient amplitude to matter are those which lie within the range of frequency variation chosen to represent full modulation. Now the need for this wide band of frequencies, $\pm$75 kc/s. for F.M. against $\pm$15 kc/s. for an ideal A.M. transmitter and very much less for the existing medium-wave A.M. transmitters, controls the choice of carrier frequency for F.M., for three reasons:

(1) The medium-wave broadcast band covers the frequency range from 550 to 1,550 kc/s.; this would provide space at most for 6 wide-band F.M. channels as against the present allocation of about 110 A.M. channels.

(2) Practically all radio apparatus employs resonant circuits, and the frequency band which can be conveniently handled by such circuits is a percentage of the centre frequency; it is therefore easier, both in transmitter and receiver, to handle a band-width of 150 kc/s. if the mean frequency is greater than the 1,000 kc/s. or so of the medium-wave band.

(3) Wide-band signals can be very seriously affected by fading. It is annoying enough for the signal strength as a whole to fade in and out, but quite intolerable distortion is likely if part of the signal fades out while another part of it gets stronger; the greater the width of frequency-band occupied by the signal, the greater the risk that fading will be "selective" in this way, and for reasons that would take us into rather extensive mathematics, F.M. is worse in this respect than A.M.

The third of these reasons turns out to be the most critical, and determines that the carrier frequency for wide-band F.M. must be above 40 Mc/s. so that there shall be no reflection from the ionosphere and therefore no fading. Certain differences between F.M. and normal medium-wave broadcasting arise from this high carrier frequency and would equally apply to an amplitude-modulation system on the same frequency: they may be summarised thus:

(1) Equal signal strength by day and night, and no fading.

(2) The service area of a transmitter extends to the optical horizon (as viewed from the transmitting aerial) and a little beyond this by diffraction, but normally there is no long-distance sky-wave transmission.

(3) The general noise level from atmospherics and electrical machinery is much lower than on medium waves, but car sparking-plugs are a noticeable source of interference (as was found with the British television service before the war).

(4) Because the wavelength is short, it is easy to focus the radiation in desired directions by setting up an "array" of aerials, the overall dimensions of the array being several times the wavelength.

Advantages of Frequency Modulation

On top of this background of characteristics imposed by the order of carrier frequency which is necessarily associated with F.M., one has to place the characteristics which are due to the system of modulation as such. An early idea about F.M. was that since many forms of interference (e.g. atmospherics) appeared to cause more or less irregular changes of amplitude of current in the receiving aerial, a transmitting and receiving system which ignored changes of amplitude and worked instead on changes of frequency of a sinusoidal carrier wave would be completely immune from this type of interference. Experimental and mathematical investigation showed that this idea was rather more simple than correct, but that as well as reducing interfering "noises" an F.M. system made it easier to separate two signals (both F.M.) on the same or adjacent carrier frequencies. For details of the mechanism of these effects reference should be made to the technical papers, but the results can be summarised as follows:

(1) The separation of two F.M. signals on the same carrier frequency requires a difference of signal strength of only 2–3 times, compared with 100 times for A.M. signals.

(2) *Impulsive interference*, such as that from car sparking-plugs, is negligible provided that the receiver is properly designed and is tuned to a signal. One way of looking at this effect is that an impulse causes the resonant circuits in the receiver to "ring" at their natural frequency, and if the receiver is correctly tuned this natural frequency corresponds to the carrier frequency of the signal, i.e. the zero-modulation point in the frequency modulation, and therefore gives no output from the detector.

(3) *Background hiss* (e.g. due to receiving valves and circuits) is greatly reduced provided that the signal is initially stronger than the hiss. This is similar to point (1), with the hiss taking the place of an interfering signal: hiss has a random waveform which may be represented by the sum of sine waves with all possible frequencies and phases and therefore must include frequency modulation as well as amplitude modulation, the latter being disregarded in the F.M. receiver and the former treated in the same way as any other interfering signal.

Arising from these characteristics, F.M. broadcasting has two potentialities not shared by A.M. broadcasting, namely, high quality of reproduction and large numbers of local broadcasting stations free from mutual interference. Taking first the question of quality, there are several reasons why high-quality receivers have not been popular in the past. First there is the technical reason that on medium waves there is a heterodyne whistle at a frequency of 9 kc/s., which means that the highest frequency reproduced by the receiver (unless it is very close to the desired transmitter) must be limited to about 8 kc/s.; this would be fairly good, but most listeners demand that their sets shall be capable of receiving foreign stations, and to separate two stations of fairly similar strength the frequency band must be restricted to 4 kc/s. which is noticeably inadequate. Second, a receiver capable of reproducing the full range of audible frequencies shows up both interference and any form of distortion produced in the receiver itself. Third, there is the purely psychological reason that it is common to use a radio set to provide "background" and the listener does not wish to devote the whole of his attention to it; a high-quality receiver compels one's attention to an extent that the mellow and mumbling set does not. It therefore appears that the real scope for high-quality receivers is for serious programmes and for the sound accompaniment of television. The latter is in a special category because it is assumed that a television programme demands the whole attention of the viewer, who will therefore want to listen to the sound accompaniment instead of merely regarding it as a not too disturbing background. F.M. is therefore especially appropriate to the broadcasting of television sound and is the standard system for that purpose in U.S.A., where it has also proved popular for ordinary broadcasrs on account of its freedom from interference.

The possibility opened up by F.M. of having large numbers of local broadcasting stations free from mutual interference has been described by E. H. Armstrong, inventor of wide-band F.M. broadcasting, as "the new radio freedom" in which no community or minority need be kept

off the air for the technical reason of lack of wavelengths, a reason which has practically compelled medium-wave broadcasts to be monopolistic. The engineering cost of a medium-power F.M. transmitter, say 1 kw., is not high, and the operating cost is largely governed by the expenditure on programme material. The first move towards exploiting this aspect of F.M. broadcasting appears to have been the establishment in New York of a group called the "People's Radio Foundation", which aims to set up a non-commercial F.M. transmitter in New York and to encourage the formation of familiar organisations in other cities. It is suggested that such a station, dedicated to free speech and public service, would counteract the political bias of the commercial networks and would also give the public true information about advertised products.

In Britain there is strong organised opinion in favour of a B.B.C. monopoly of all broadcasting, though the pre-war commercial programmes from the Continent were very popular and a Gallup Poll taken during the war indicated that about half the general public approved of commercial broadcasting. Possibly in Britain we might have a compromise on the following lines. The sound part of the television programme ought to be on F.M., and during the remainder of the day (assuming that for some time to come the television will only be on for two or three hours a day) the television sound transmitters in all regions should radiate a "National" programme made up of those items from the available programmes which are the best of their kind. In addition there should be several "local" transmitters in each region open, to a much greater extent than are the present B.B.C. stations, to local musical and dramatic talent, talks on local subjects, uncensored five-minute talks by the exponents of all kinds of "isms", some sponsored entertainments, and any necessary filling out by selections from the other B.B.C. programmes or by the B.B.C.'s war-time technique of "personal choice" gramophone record sessions of one kind or another. Such local stations should, in fact, be related to broadcasting as we know it to-day in the same way as local newspapers are related to the national daily press, and the local broadcast talks sessions should be as catholic in acceptance of material as are the correspondence columns of most local papers.

However, this discussion of programme policy is something of a digression. The object of this article has been to explain from the engineer's point of view what F.M. is and how it can offer a service which differs from broadcasting as we know it to-day, but the use to be made of that service must be decided by the listener: you, the reader, must decide what should be the future of British broadcasting in the light of technical progress.

READING LIST

"Frequency Modulation Noise Characteristics", M. G. Crosby, *Proc. I.R.E.*, 1937, Vol. 25, p. 472.

"NBC Frequency Modulation Field Test", R. F. Guy and R. M. Morris, *R.C.A. Rev.*, 1940-1, Vol. 5, p. 190.

"F.M. Communication System", D. A. Bell, *Wireless Engineer*, 1943, Vol. 20, p. 233.

"The New Radio Freedom", E. H. Armstrong, *Journ. Franklin Inst.*, 1941, Vol. 232, p. 213.

Radio and Army Education

THE programmes which the B.B.C. are presenting daily in connection with the Forces Educational Scheme promise to prove of great service in supplementing the work of the Unit instructors (writes our Forces Correspondent). The programmes are normally talks or discussions, covering subjects included in the scheme, and range from hints on washing clothes to an account of the history and achievements of the electrical industry.

Naturally some subjects are more suitable than others for exposition by broadcast talks. The various speakers on current affairs have fulfilled by their lectures a need which otherwise would have been difficult to meet. And most speakers have been wise enough to limit their talks to the presentation of a general background to a subject instead of attempting the impossible task of dealing with it exhaustively.

In this connection I would single out for special praise the series of talks given by Professor D. M. S. Watson on "Man's Place in Nature". These talks covered the development of man; the emergence of life on this planet, the development of man's social and gregarious instincts, the influences which conditioned his habits, and the specialisation of function in society. These talks, which were presented quite objectively with a mass of factual detail, served as an excellent introduction to a more detailed study of the subject.

Just as valuable were the five dramatic presentations, written and produced by Nesta Pain, of important scientific discoveries. Presented in non-technical language, they gave the history of the war against sleeping sickness and yellow fever, and finished with an account of the splitting of the atom and uranium fission.

If there is one defect from which the Forces Educational Scheme suffers, it is that it tends to make the acquisition of knowledge seem too easy. In its eagerness to interest, it tends to gloss over the hard work required for the mastery of any subject. The programmes presented by the B.B.C. do something to remedy this lack of balance. They introduce a subject, they ask questions which the listener can solve only by research of his own. They have already been welcomed and widely used, and it is to be hoped that their scope will be extended still farther.

The B.B.C is inviting men and women in the Services to take a more direct part in the planning of the Forces Educational Broadcasts. Here is an announcement for Service listeners, reproduced from the *Radio Times*:

"If a famous scientist or historian or economist came to your unit, what questions would you fire at him? What music would you ask to hear analysed and performed if you could bring the musicians to the class-room? What books would you like to be read or dramatised? What programme repeated or developed? A whole week's F.E.B. programmes will be built round the answers to these questions. Envelopes should be marked 'F.E.B. Requests' and addressed to: Manager, Services Educational Unit, B.B.C., London, W.1." Request Week is November 26—December 1. If the scheme is a success there will be other Request Weeks at six-weekly intervals.

Scientific Observation in 1700

THE history of science is a fascinating subject which has suffered much at the hands of writers of short articles, who have tried in a few words to show how scientific progress is linked with human needs, or how new theories grow out of the remains of old ones. In order to redress the balance I propose to delve a little into the mass of day-to-day scientific work which is usually ignored by historians whose chief concern is with the broader issues.

Towards the end of the seventeenth century the various scientific societies which had been formed in Europe—the Accedemia dei Lincei in Rome, the Royal Academy of Sciences in Paris and the Royal Society in London—began to publish the papers which were read before them, and the minutes of their proceedings. It is perhaps in these minutes that the best picture of contemporary scientific thought is to be found, for curious happenings in all parts of the world were reported at the meetings and the learned members would offer explanations in the light of their own knowledge and philosophy whenever they could.

As a fairly typical example let us examine the contents of the *History and Memoirs of the Royal Academy of Sciences at Paris*, for the year 1700. (The term "history" in the title means the Academy's minutes). These records were translated into English in 1741 by John Martyn, F.R.S., Professor of Botany at the University of Cambridge.

The contents of the *History* included:

I. An observation of the barometer, thermometer, and quantity of rain and snow water that fell at Paris in the Royal Observatory during the year 1699, by *M. de la Hire*.

II. A new manner of rendering barometers luminous, by *M. Bernoully*, professor at Groninguen.

III. Remarks on the construction of pendulum clocks, by *M. de la Hire*.

IV. An extract of some letters written from Portugal and Brazil by *M. Couplet* the son, to the *Abbé Bignon*, president of the Royal Academy of Sciences.

V. A general method of throwing bombs, in all cases proposed, universal instrument for this purpose, by *M. de la Hire*.

The *Memoirs* comprised the following items:

I. On some singularities of France. II. On the apparent largeness of the horizontal moon. III. On a body of fire seen in Normandy. IV. On the yellow amber. V. On a change made in the texture of bodies by external motion only. VI. On the trembling of the nerves of a frog after death. VII. Of a stone found in the bladder of a mare. VIII. Of a monstrous double leveret. IX. On a new telescope glass. X. A false report of the perpetual motion being discovered, and the impossibility of it demonstrated.

The discussion on the apparent size of the moon when near the horizon starts off by confessing that "this phenomenon has very much embarrassed the greatest philosophers among the moderns, as it often happens that when we give very different explications of one and the same thing none of them is true." What follows does little to clear the matter up, concluding that it is due to an optical illusion similar to that which makes fluted columns look larger than plain ones of the same size. It must be admitted, however, that this problem is still being discussed to-day, and most of the arguments used are the same as those put forward in 1700.

The change in texture as a result of external motion was communicated by a *M. Homberg* who "says that, having tied a bottle of wine to the clapper of a mill, he found that the motion of this clapper alone had changed the wine into very good vinegar in the space of three days; and that a pound of quicksilver had by the same means in three months time given 4 or 5 ounces of a blackish powder." This the Academy find most surprising, but they have a ready explanation for the trembling of the legs of a recently killed frog that occurred when the nerves leading to them were irritated with a scalpel. "If the frog had been longer dead," they say "this would not have happened. In all probability there yet remained some liquor in these nerves, the undulations of which caused the trembling of the parts where they corresponded, and consequently the nerves are only pipes, the effect whereof depends on the liquor they contain."

The report of perpetual motion is dismissed very briefly, it being noted that "a little while after the noise that this discovery made, the perpetual motion disappeared with its author." Nevertheless plenty of tricksters have been able to make a good living out of "perpetual motion machines" since *M. Parent* of the Academy demonstrated their impossibility in 1700.

M. de la Hire, who as the Astronomer Royal was one of the few paid professional scientists of the day, outside the universities, kept regular meteorological records over a number of years. Among his instruments, which were of course of his own construction, were a mercury barometer, an alcohol thermometer and a home-made compass needle with which he recorded the variations in magnetic declination.

In 1675 M. Picard, a predecessor of de la Hire's in the Observatory at Paris, published a treatise on barometers in which he recorded that when he carried his barometer down to the cellar of the observatory the chance motion of the mercury at the top of the tube caused an intermittent light to appear in the space above it. *M. Bernoully*, one of the famous Swiss family of mathematicians, decided to investigate this phenomenon because it appeared that not all barometers showed it. In this paper he demonstrated that it is essential that the mercury and the glass tube are absolutely clean and dry for the light to be observed; but the Academicians were not convinced, asserting, since they could not repeat his results, that he must have used a particular sort of mercury which differed from their own. This led to a series of letters from Bernoully, in which he patiently explains to them where they went wrong in their experiments, and finally "desires the Academy to send me some of the same quicksilver which they have used without success", with which he "engaged to make a *phosphorus* . . . as good as those which I have made hitherto." This account is interesting as being the first record of man-made luminescence in a gas, the fore-runner of the neon sign and the modern fluorescent lamp. Bernoully has an explanation of the phenomenon (in terms of "subtile matter" which comes out from between the pores of the mercury) which derived from Descarte's theory of the origin of light.

The letters of *M. Couplet* are chiefly concerned with his determinations of the latitude and longitude of Lisbon and of Paraiba in Brazil.

After browsing through these pages one cannot help feeling exhilarated by the atmosphere of excitement and expectation which in them is far closer to the surface than is the case for any modern scientific journal.

C. G. A. HILL.

Night Sky in December

The Moon.—New moon occurs on December 4d. 18h. 06m. U.T., and full moon on December 19d. 02h. 17m. The following conjunctions take place:

Dec.			
3d. 09h.	Venus in conjunction with the moon	Venus	2° S.
20d. 21h	Saturn ,,	Saturn	2 S.
21d. 12h.	Mars ,,	Mars	0·7 N.
27d. 21h.	Jupiter ,,	Jupiter	4 S.

In addition to these conjunctions with the moon, Mercury is in conjunction with Venus on December 13d. 04h., Mercury being 2°·1 N.

The Planets.—Mercury is in inferior conjunction with the sun on December 7 and is stationary on December 17. The planet sets at 16h. 30m. at the beginning of the month—about 35 minutes after sunset, and rises about 1½ hours before the sun in the middle and end of the month. Venus can be seen in the late morning hours, rising at 6h. 22m., 7h. 04m. and 7h. 42m. at the beginning, middle and end of the month respectively.

Mars, in constellation of Cancer, is conspicuous during the night, rising at 19h. 45 m.,18h. 43m., and 17h. 13m. at the beginning, middle and end of the month respectively. The planet is stationary on December 5. At the beginning of the month Mars is 73 million miles from the earth and at the end of the month the distance has decreased to a little over 60 million miles.

Jupiter, in the constellation of Virgo, a little north of α Virginis, is visible in the early morning hours. The times of rising for the beginning, middle and end of the month are 3h. 11m., 2h. 30 m., and 1h. 38m. The distance of Jupiter from the earth varies between 564 and 525 million miles from December 1 to December 31.

Saturn, in the constellation of Gemini, is visible in the evening hours, rising at 19h. 11m., 18h. 12m., and 17h. 02m. at the beginning, middle and end of the month, respectively. The distance of the planet from the earth is 930 million miles on December 1 and 753 million miles on December 31.

Times of rising and setting of the sun and moon are given below, the latitude of Greenwich being assumed:

Dec.	Sunrise	Sunset
1	7h. 43m.	15h. 55m.
15	7h. 59m.	15h. 50m.
31	8h. 06m.	16h. 00m.

Dec.	Moonrise	Moonset
1	3h. 55m.	14h. 49m.
15	13h. 55m.	2h. 48m.
31	5h. 04m.	13h. 57m.

Eclipse of the Moon.—There will be a total eclipse of the moon on December 18-19, visible at Greenwich. The circumstances of the eclipse are as follows:

December	
18d. 23h. 38·4m.	Moon enters penumbra
19d. 00h. 37·5m.	Moon enters umbra
19d. 01h. 40·5m.	Total eclipse begins
19d. 02h. 20·3m.	Middle of eclipse
19d. 03h. 00·2m.	Total eclipse ends
19d. 04h. 03·1m.	Moon leaves umbra
19d. 05h. 02·3m.	Moon leaves penumbra

Many interesting objeets in the heavens can be studied during the winter months with the aid of a small telescope or even with the naked eye. The Constellation of Taurus contains the well-known star α Tauri, generally known as Aldebaran, which is of a bright red colour. Its distance from the earth is about 57 light-years and its diameter is 60 times that of the sun, or 52 million miles. Its surface temperature is only about one-half that of the sun—less that 3,000° C, but owing to its enormous size its luminosity is very much greater than that of the sun. The beautiful star-cluster named the Pleiades has always attracted attention. A person of average sight can see six stars of this cluster with the naked eye, but those with a keen sight can see more, and any who wish to test their sight should try to count the number of stars that are visible. A pair of binoculars will reveal a large number invisible to the naked eye and a small telescope will show still more. The Pleiades cluster is surrounded by nebulous matter which shines by reflecting the light of stars. M. DAVIDSON.

JUNIOR SCIENCE

About Atoms—II

WHEN the ancient Greek philosophers first hit on the idea that all matter might be composed of atoms, that is, small indivisible particles, they thought of these atoms as being tiny bits of matter which differ from the larger bits only in size. They thought, for instance, that if a piece of iron is broken in half, and then each half is broken in half again, and so on, in the end there would be very small bits of iron which could not be divided further. These tiny fragments, the atoms, would still be lumps of iron with all the properties of an ordinary lump of iron except that they could not be broken up into still smaller pieces. However, it was realised that the iron atoms must have some means of holding on to each other and thereby giving the piece of iron its great strength. The Greeks thought that the atoms had little hooks on them or that they might fit together like the pieces of a jig-saw puzzle.

After Newton had explained that the sun and the planets are held in place by the invisible force of gravity, scientists tried to interpret the cohesion of atoms and molecules in much the same way. But force of gravity is certainly far too small to account for the strength with which the atoms in a lump of iron or in a stone are kept together. It was then suggested that the small particles which go to make up things as we know them must have strong electric charges. You remember that in an atom *negative* electrons circle around a *positively* charged nucleus. But even this explanation soon proved insufficient. Why, for instance, should the different iron atoms stick together, as we know they do, when they are all negatively charged outside and should by rights repel each other?

That question and others equally awkward went to show that something about our picture of atoms and electrons was completely wrong. The real root of all these troubles is that we imagine a very small bit of matter like a proton or an electron to behave in much the same way as the large bits which we can see and feel, such as billiard ball, an apple or a grain of sand. For a long time scientists thought of electrons as of miniature billiard balls which are electrically charged and which circle around the nucleus just as the earth, which we can regard as a very large billiard ball, circles around the sun, a still larger billiard ball. But here we see straight away how senseless a model can become once we apply it to different dimensions. If the sun and the earth were to collide, they would not bounce off each other like billiard balls; indeed the earth would break up and take a different shape before even the sun and earth could meet; in the end the earth would become a bit of the sun.

Since things much bigger than billiard balls behave quite differently from billiard balls, differences in behaviour may be expected with things much smaller than billiard balls. That is, in fact, found to be the case. But while we can see billiard balls and things like the sun and the moon and so form an idea as to their nature, we cannot see protons or electrons. With all the power of human imagination we will therefore never be able to form a true picture of the nature of these particles, just as we cannot form a picture of the force of gravity. However, we can describe the action of gravity by a mathematical formula (by saying that the attraction between two bodies is inversely proportional to the square of the distance between them) and we can similarly describe the properties and behaviour of sub-atomic particles in mathematical terms. If we do this, we find that iron atoms must attract each other although they are negatively charged outside; this is possible because the electrons in them are not just tiny negatively charged spheres but have certain other properties which we can calculate but not comprehend. This inability to comprehend things need not worry us; we cannot really comprehend what pulls us to the ground when we stumble but we are quite accustomed to putting up with this fact.

K. M.

The Bookshelf

Trees and Toadstools. By M. C. Rayner. (London, Faber, 1945; 71 pp., 18 plates; 6*s.*)

DR. RAYNER has worked for many years on the relations between flowering plants and fungi and, as her botanical colleagues know well, she has made substantial contributions to our still incomplete appreciation of the very important biological connexions that exist between these two markedly distinct groups of organisms. In *Trees and Toadstools* much information hitherto available only to the practising botanist is brought together for the benefit of the non-botanical reader, to whom, as maybe to the botanist, the full story will come as an interesting surprise.

The book is well produced and generously illustrated by really good, informative pictures. The subject matter is presented in such a way that any reader should follow the development of the story with ease and comprehension. Here and there, perhaps, the author's mastery of her subject may have led her to assume more familiarity with the habit of thought of the experienced botanist than some general readers may possess, but even so the attentive reader will find nothing left unexplained. One small point—we might have been spared "in the case of" altogether.

Trees and Toadstools earns a special welcome because the author is so wholeheartedly interested in the living plant and in the conditions which surround the living plant. The book is quite free of the atmosphere of the herbarium and the laboratory; it carries no lingering aroma either of dried plants or of methylated spirit. When botany was young, and when a vast amount of information had to be put into order, then it seems to have been inevitable that the herbarium and the laboratory should make the environment in which the botanist worked. That phase has passed. We still must have herbaria and laboratories, for we must still be able to identify plants accurately, we must continue to investigate the structure and behaviour of plants, and, so long as students have to prove their worth by passing examinations—and there seems no prospect that the formal examination can be replaced by anything more satisfactory—the herbarium and the laboratory will rightly retain their places. But the real fortune of botany is to be sought out-of-doors, among living plants, whether cultivated or wild. Dr. Rayner's book sends our thoughts at any rate into the proper place to search for fortune.

B. BARNES.

The National Trust. A Record of Fifty Years' Achievement. (London, Batsford, 1945; xii+132 pp.; 12*s.* 6*d.*)

THERE was once an American soldier stationed in Salisbury, whose idea of improving the amenities of the Close was to demolish the surrounding buildings and to whitewash the exterior of the cathedral. Anyone sharing his notions will probably not be a supporter of the National Trust; but almost anybody else will agree that it is performing a vitally necessary function.

In point of fact, few ordinary citizens until they have read this admirable volume will have any real idea of what a complicated business the work of the National Trust has become; and though (as is freely admitted) it is not really possible to divide the Trust's properties into watertight compartments, nevertheless the ten sections into which this fifty years' record is divided give the ordinary reader a particularly good idea of the difficulties facing the management of the Trust. A further advantage is gained by the fact that every section is by a different author, each of whom holds a slightly different point of view. The present state of affairs is summed up in an excellent preface by Professor G. M. Trevelyan, O.M., who is not only chairman of the Estates Committee of the National Trust, but also the most distinguished living historian in England; while an admirably concise review of the Trust's actual work is contributed by its capable and energetic secretary, Mr. D. M. Matheson.

The appearance of this volume is timely indeed: never was there greater danger of the Englishman's heritage in England being stolen from him by sectional interests. There is, indeed, talk of State support for National Parks—a scheme not incompatible with the Trust's ideals—but nothing concrete has yet been done. The average man has an uneasy feeling that planning of the countryside is being done above his head and behind his back, and that he will wake up one morning and find that irreparable damage has been wrought during the night. The Trust is the average man's watchdog in these matters, and though a watchdog cannot do everything, it can keep off some marauders at any rate.

How admirably the Trust has so far achieved its purpose can well be gathered from the pages of this volume. Founded at a time when economic cash values were still the official standard for land utilisation, the Trust has shown how important to our national well-being are the imponderables—the inspiration that town-dwellers can derive from contact with the beautiful things of the countryside, or indeed from association with "shrines" in their own streets. It is hard to assess the value of "Historic Interest and Natural Beauty" on the credit side of the national balance sheet, but an ever-growing number of citizens are quite sure that they do, in fact, have a great and increasing value.

The National Trust conserves, but it does not preserve its properties like specimens in alcohol. It has no State subsidy, so that its properties have to be self-supporting—the estates by the usual agricultural methods; the buildings, etc., by admission fees; and the whole organisation by the subscriptions of members. A membership entrance form, by the way, is tactfully inserted into each copy of the book under review. *Verb. sap.*

The various chapters show, cunningly indeed, how the Trust holds properties representative of every period of British history from the Stone Age to the Pre-Raphaelite period. Buildings of every kind, from the princely mansion to the humble cottage, are described. And, finally, the Trust's properties are illustrated by a really magnificent series of photographs from innumerable sources—a glance at them is sufficient to convince the most sceptical of the catholicity of the Trust's organisation. (The description of the illustration showing the Priest's House at Small Hythe is a little misleading, however, as this house, although on Ellen Terry's property, was never actually her home. Her house, a building in very similar style, lies a little farther down the road.) This volume, a book of truly national importance, is produced in a manner worthy of its subject—a Batsford book at its best. Even the loose cover has a special interest of its own.

L. R. MUIRHEAD.

Life and Work of John Tyndall. By Professor A. S. Eve and C. H. Creasey. (London, Macmillan, 1946; pp. 403+15 plates; 21*s.*)

ONE cannot help agreeing with the critics who find this book lacking in shape and literary graces (the best writing in the book is certainly Tyndall's own and the verbal mortar in which his words are embedded seems to have been daubed on with a clumsiness that suggests a lack of craftman's pride). The form of the book is not that of an integrated biography but of what, for want of a better term, may be called "life and letters". This is not altogether surprising for it is over fifty years since Tyndall died (the reason for the delayed appearance of the biography is explained in the preface) and the material upon which it is based has passed through several pairs of hands. This material reached Professor Eve (whom readers will recall was the author of the "official" Rutherford biography) in 1939, but illness forced him to pass on his first draft to Mr. Creasey for revision and completion. Nothing in the way of significant facts seems to have been lost in the process of transfer; the compilation has clearly been painstaking, each fact being treasured as reverently as the talents that were buried by a character in a biblical parable. A person sensitive to words can usually tell whether a book has been written by hand or on a typewriter, whether it has been the work of an individual or of a committee. The odd circumstances connected with the writing of this book will be obtrusively obvious to any such person, who will derive but little pleasure from the mixture of literary styles. Such mixing of styles was not inevitable in this instance, though to avoid it might have entailed a still further delay before the book could be published.

Such is the impression obtained from a single reading of the book, though one

realises very quickly that it is almost certainly bound to remain the standard life of Tyndall, for it seems unlikely that any more detailed biography will come to be written. But a second or third reading —there is something about the book, probably Tyndall's personality which emerges in spite of the way the book was put together, which encourages one to return to it—leaves one with a more favourable impression. It is then possible to ignore the many irritating faults that mar one's first reading.

In a way the book reminds one of the kind of portrait which a scientific-minded keeper of an art gallery submits to an X-ray examination, to find that the canvas carries two or three pictures and not just one. A bold portrait, which is largely Tyndall's self-portrait, stands out from the pages of this book if one takes plenty of time to study them. The background of the portrait proves to be equally worth studying. Science in the days of Tyndall and Huxley was very different from the present day when the scientific profession is an extremely competitive one (some people consider it the *most* competitive profession in the world). There was pride in discovery, naturally, but among the greatest scientists of that period the pride was tinged with humility. Competition being less fierce than to-day, Tydall was able to spend much time championing other scientists who had been neglected. It is inspiring to read of what he did for Mayer, and of the way in which he helped Hooker, Rendu and Agassiz when they were in difficulties. He might have been a greater scientist had he let science monopolise his life, but then as a man he would have been that much the smaller and we should not be troubling to read his biography in 1945.

Science was less specialised then, and it is hard to imagine a modern physicist contributing to biology as Tyndall did. For it can be said with full justification that "along with Pasteur's essays the work of Tyndall constitutes the foundation of all subsequent work on bacteriology". (It is odd that the word *pasteurisation* has become part of the English language, whereas *tyndallisation*—sterilisation by discontinuous heating—has not. The latter word is practically unpronounceable, however, which may account for it not being acceptable to the layman.)

There is a short but most interesting chapter on Tyndall as a populariser. In that field he was pre-eminent; the Royal Institution has produced a distinguished line of popular expositors, including Davy, Faraday, T. H. Huxley and Sir William Bragg, but Tyndall was second to none. He could attract not only a Royal Institution audience; one reads of working-class audiences of six hundred who paid five shillings for a course of ten lectures by Tyndall. He "knew that a public lecture should have the same exacting care in production as a play in a theatre". Tyndall aroused plenty of jealousy by his success as a popular lecturer and writer; "Dr. Tyndall", wrote one critic, "martyred his scientific authority by deservedly winning distinction in the popular field". Yet Tyndall's reputation as a scientist has stood the test of time far better than that of his critic, while his writings are still very much worth studying for the brilliant imagery and for the beautiful touches in his selection of metaphor and simile.

One sets out to express high dudgeon at the lack of sensitivity shown by the biographers, and ends up by recommending the book to all who enjoy biographies and particularly to those who make a special study of the history of science.

WM. E. DICK.

Chemical Crystallography. By. C. W. Bunn. (London, Oxford University Press, 1945; 422 pp., 25*s*.)

MANY books have been written on the subject of X-ray diffraction since its discovery about thirty years ago, and they have usually followed the same sort of pattern—a section on crystals, a section on X-rays, sections on the different kinds of X-ray photographs, and a section on results. The last part, has of course, been the most important, but for the first sections authors have mainly been content with repeating already-published diagrams and photographs until some of them have been almost boringly standardised. It is refreshing, therefore, to find a completely different approach is used in *Chemical Crystallography*; no old diagrams are introduced, and new methods of exposition are advanced.

It may be that the new approach is no improvement over the standard, or that the new diagrams and expositions are no clearer than the old ones; the point is that the reader is made to look at familiar problems in a new light, and this cannot but lead to good results.

Mr. Bunn's treatments, however, *are* often an improvement over the established ones. For example, consider the derivation of Bragg's law. It is usually considered that the idea of reflection from the lattice planes is so obvious that no discussion of it is needed. That this is not so is shown by the careful treatment by Bragg in his original papers, and Mr. Bunn has also given this question the importance it deserves. The proof given (pp. 116 and 117) may not be very elegant, but it does achieve its purpose of impressing the reader with importance of the concept of "reflection" of X-rays from the lattice planes. The treatment of the indices h, k, l, of the reflections (p. 131) is also treated in an original manner, which is a great improvement over explanations that stop at the correspondence with the Miller indices of the reflecting planes.

The optical properties of crystals are very fully treated, and on this account the book should be very valuable to those who have entered the subject without an adequate training in crystal optics. The accentuation is on the practical side of the subject, and the advantages and limitations of the various methods are clearly described. Mr. Bunn has taken great pains to make the subject intelligible to the beginner, and in this has probably succeeded. Some of the diagrams (e.g. Fig. 47) are, perhaps, too complicated to be of great use until the subject has been learned from the text, but otherwise the treatment is to be commended.

Most of the book is devoted to the principles of the art of structure determination, to which Mr. Bunn has made many valuable contributions. For the first time one finds detailed accounts of the structure-factor graphs and of the "fly's eye", with adequate explanation and illustration. The section on Fourier series, however, is not as complete as it might have been. Fourier methods are used in two different ways: first, to render more exact the atomic positions found by trial-and-error methods; secondly, to derive atomic positions directly without any previous knowledge or assumptions. The former aspect is adequately dealt with, but the latter is confined to crystals that have a heavy atom at a centre of symmetry (p. 342). This, of course, is a rare occurrence (phthalocyanine is likely to remain the sole example for some time) and one would have welcomed a discussion of Fourier series used in more general cases. This omission is partly rectified in the section on Patterson series, but much useful work was done before the Patterson method came into general use.

A final point on which the author is to be congratulated is the index. The index of the book is often considered as unimportant, and is compiled with insufficient care. Nevertheless, everyone must at some time or other felt the irritation that results from inability to find, for example, a particular reference in a book. Mr. Bunn has produced a name index that is a model for other authors to copy; it has been combined with the references, so that, in addition to fulfilling the usual functions of an index, it enables one to find the page on which a given paper is referred to. The subject index also appears to have been compiled with great care, though, of course, in this there is less scope for originality.

To summarise, the book must be regarded as an important addition to the literature and should be useful both to the young research worker and to the more advanced. The care with which it has been produced and the absence of important mistakes should also make it a valuable book of reference.

H. LIPSON.

X-Ray Metallography. By A. Taylor. (London, Chapman and Hall, 1945; 400 pp., 36*s*.)

X-RAY diffraction methods are now recognised as essential to the fundamental study of metals, and they also play a large part in many practical investigations; the publication of a text-book on the subject is therefore greatly to be welcomed. Dr. Taylor's book is well thought out and a careful collection of material from published papers so it should form a valuable book of reference for those working in the field.

Dr. Taylor claims that his book should also serve as a student's text-book, but it is doubtful whether it will fill this role successfully. In the first place, the terminology has not been kept strictly consistent, a grave defect in a book intended

for beginners: secondly, the presentation of some subjects is rather perfunctory: and thirdly, the reviews of some aspects of recent work are rather uncritical and do not attempt to sift the wheat from the chaff. These criticisms are not put forward in a carping spirit; indeed, Dr. Taylor has been a great deal more careful in avoiding the incorrect use of terms than the authors of several other recent works on the subject and the changes necessary to make his book accurate in this respect are not many.

Of particular importance is the use of the word "lattice". In crystallography this has a very definite meaning; it indicates the type of scaffolding on which the structure is based. The lattice has no real existence; it is merely an abstraction which is useful for providing a basis for the description of crystal structures and for serving as an introduction to the theory of X-ray diffraction. A crystal structure can be described by associating with each lattice point a number of atoms grouped in a particular way. Now it so happens that most of the important metallic structures have only one atom associated with each lattice point and thus the lattice points are identical with atomic centres. From this special fact the legend has grown up, particularly amongst those dealing with metals, that the words "lattice" and "structure" are interchangeable. Two American writers have recently protested against this practice, and Dr. Taylor has, on the whole, not fallen a victim to it. But on pp. 38 and 266 he refers to the "hexagonal close-packed lattice". The hexagonal close-packed *structure* is derived by attaching two atoms to each point of a hexagonal lattice—there is no close-packed hexagonal lattice. Similarly, in the tables on pp. 382-386, the "lattice type" of each element is given, when obviously the "structure type" (used correctly in an earlier table) is meant. For example, silver and diamond have the same lattice but different structures.

In addition, it is rather a pity that the Shoenflies point- and space-group nomenclature has been used: Dr. Taylor himself says that "It is now being rapidly superseded by the more descriptive symbolism devised by Hermann and Mauguin" (p. 388).

The second criticism—that of inadequate presentation—does not apply to most of the chapters, since it is obvious that particular pains have been taken to make the subject clear. But it is doubtful if the structure amplitude formula should be presented *only* in terms of complex quantities (pp. 83, 91, 92): the formulae given are certainly in the neatest forms, but the expressions in terms of cosines and sines are more easily understood and certainly have to be used in practice. Also, Miller indices h, k, l, are first introduced by means of numbers H, K, L, proportional to their reciprocals. This is rather unusual, and has not a great deal to commend it. For instance, if h is zero, H is infinity, and so can hardly be called a "small whole number" (p. 27).

The criticism that the reviews of recent work are to some extent uncritical is based chiefly on the section on the deformation of crystals. Dr. Taylor, at the top of p. 229, gives a clear theory of the cause of the broadening of the X-ray reflections from deformed metals, but then goes on apparently to accept others such as that of "limiting crystallite size". Consequently the reader is left in doubt about which theory is being put forward. Recent work leaves no doubt that the former—Dr. Taylor's own—is correct, and it certainly provides a simpler basis for theories of the strength of metals.

Dr. Taylor's book should be much more useful as a work of reference than as a student's text-book, but it could easily be made suitable for the second purpose by a certain amount of revision; it is hoped that a second edition will make this revision possible. May the suggestion also be made that the chapter on radiography should be omitted and that the index should be made more extensive? One misses the names of Schoenflies and Mauguin, and it should not be necessary to have to find the name of Waller from the fact that it is associated with that of Debye.

H. LIPSON.

Far and Near

Atomic Research Station

IT was announced by the Prime Minister on October 29 that the Government had decided to set up a research and experimental station to embrace all aspects of the use of atomic research. It will be situated at Harwell airfield, near Didcot. Control of atomic research has now been transferred from the Department of Scientific and Industrial Research to the Ministry of Supply. The Directorate of Tube Alloys (the camouflage name for Britain's atomic research project) accordingly becomes part of that ministry.

An International Congress in Miniature

AFTER the Great War it was nine years before geneticists were able to organise an international congress. Less than ten weeks after VJ Day a special meeting of the Genetical Society was held in London which merited the description given to it by the president, Dr. C. D. Darlington, in his opening remarks—"a small international genetics conference". Travel facilities had been secured by the British Council to enable geneticists of several nations to attend (the French Scientific Mission made the arrangements for their countrymen), and the Royal Society cooperated with a Pilgrim Trust Lecture from H. J. Muller, Professor of Zoology at Indiana University. There was some disappointment that no Russian delegation was able to be present, but Professor Haldane said that invitations did not reach Moscow until the end of September. The opening lectures, by Dr. Darlington and Professor Haldane, gave a summary of British advances made in genetics during the war. Other papers were given by Professors Mohr, Hagedoorn, Bonnier, Winge, Muntzing and Caspersson, and Drs. Sirks, Ephrussi, Brachet, Harland and Teissier.

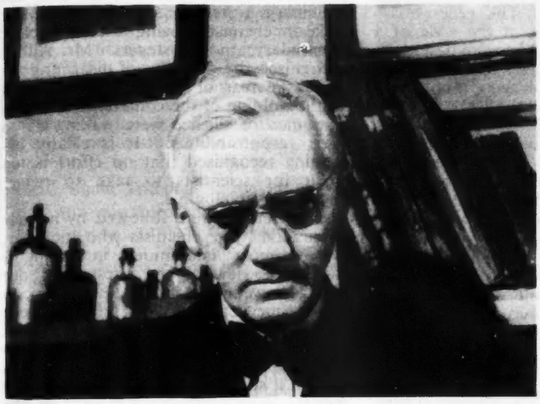
Government Science Proposals Criticised

CRITICISM of the Government's proposals for improving the salaries of Government scientists is voiced in *State Service* (organ of the Institution of Professional Civil Servants, the body to which a large proportion of Civil Service scientists belong). In an article on "The White Papers", the secretary, L. A. C. Herbert, comments: *It has been clear for some time that the salaries paid to the heads of the professional, scientific and technical divisions were far too low, but the Treasury could not introduce major improvements without compromising its first Article of Faith, the absolute superiority of the Administrative Class. To put it bluntly, the "floor" for the top administratives had to be the "ceiling" for non-administratives. The floor has now moved up a little and the ceiling below has moved with it.* (This fact emerged clearly from the graph published in last month's DISCOVERY —Ed.)

Mr. Herbert welcomes the appointment of a scientist to the Civil Service Commission, and says that the most important organisational improvement is the introduction of central recruitment "in place of the haphazard hole-in-corner recruitment by Departments. He states categorically that the Institution of Professional Civil Servants has not accepted the new salary scales because it "cannot acquiesce in the continued inferiority in remuneration of the Government Scientific Service as compared with the Administrative and Executive Services."

According to a statement to the House of Commons, the new salary scales come into operation from January 1 next.

DISCOVERY has received the following account of the steps that preceded the appearance of the Government White Paper, and it provides some explanation for the perfunctory manner in which the proposals have been presented to the scientists and to Parliament. The original draft for the White Paper was written by a woman Civil Servant (whose competence need not be questioned), who had been lent to the Treasury by the Ministry of Health. It was intended to publish the White Paper in August, but the return of a Labour Government caused some hitch and the transfer of the author of the draft White Paper back to her old department. The White Paper as published was produced by the Treasury official who was not conversant with the negotiations between his department and the Institution of Professional Civil Servants; indeed his only acquaintance with the subject came from a reading of the draft drawn up by the Ministry of Health official. The Chancellor of the Exchequer



SIR ALEXANDER FLEMING

SIR HOWARD FLOREY

This year's Nobel Prize for Medicine represents international recognition of the importance of the fundamental research work carried out in Britain on penicillin. To DISCOVERY'S *readers the names of Sir Alexander Fleming (Professor of Bacteriology in London University and director of the Bacteriology Department of St. Mary's Hospital, Paddington, and Sir Howard Florey (Australian Rhodes scholar who is head of the Sir William Dunn School of Pathology) Oxford University) need no introduction. Dr. Chain, on the other hand, has remained in obscurity in spite of the dominant role he played in the chemical re-investigation of penicillin that began at Oxford in 1938. Born in Berlin in 1906, he is of Russian descent and son of an industrial chemist. Forced to leave Germany when the Nazis seized power, he came to Britain in 1933 and is now a naturalised British citizen. He joined Florey's team in 1935, to make many original contributions to the work of isolating penicillin and determining its chemical constitution, as well as to the recent attempts at synthesis. He has also found time to take an active part in studying other anti-bacterial mould products. (Photographs taken from the film, "Penicillin").*

DR. ERNST BORIS CHAIN

did not, it appears, give any deep consideration to the White Paper before it was printed. It seems to have been passed to the Chancellor, who laid it before the House, as though it were a *fait accompli*, which Government scientists are now expected to accept in the same spirit. It is widely felt that unless some further consideration is given to the matter by the Government that the future of the Government scientific service will be jeopardised. We understand that there is a certain reluctance on the part of the Chancellor of the Exchequer to meet a delegation representing Civil Service scientists, though it is to be hoped that he will hear their case before attempting to implement the Treasury report.

Centenary of Imperial College

AMIDST the aftermath of the Napoleonic Wars and at the dawn of the Industrial Age the Royal College of Chemistry was founded. At the end of the second World War and at the start of the Age of Atomic Energy its centenary has been celebrated by the Imperial College of Science and Technology of which the Royal College of Chemistry's descendant, the Royal College of Science, is a constituent body. Chief event in the celebrations was a meeting at the Albert Hall of more than 5000 people, which the King and Queen attended.

The extremely fine exhibition held in the buildings of the three constituent colleges, Royal College of Science, Royal School of Mines and City and Guilds, during the centenary celebrations, illustrating both the teaching methods of the college and the research work undertaken, showed how ably Imperial College fulfils the obligation laid down in its charter to provide the highest specialised instruction and the most advanced scientific training and research, especially in its application to industry.

Colour Vision of Insects

THE beautiful colouring of many butterflies and their obvious preference for highly coloured flowers may lead us to assume that they can perceive colours in the same way that we can. This assumption, however, is quite unjustified, and it is only as a result of very painstaking experiments that we can learn whether insects can distinguish colours at all, or whether, like dogs, they are completely colour blind. Classic studies of the bee's colour sense were made by K. von Frisch about 30 years ago, and his work stimulated other investigators to experiment with other insects, including the humming bird hawk-moth and the humble-bee fly *Bombylius fuliginosus*.

Dr. D. Ilse recently showed some colour films to the Physical Society demonstrating how she had been able to establish that the familiar cabbage-white butterflies can distinguish between three different regions of the spectrum. She had been at great pains to eliminate differences in brightness and in the textures and odours of her test patches of colour which would otherwise have confused the results. The experiments depended on the fact that at different periods in the insect's life it performs certain unmistakable instinctive actions—such as unrolling its tongue when feeding and drumming with its forelegs before egg-laying—which are performed preferentially on surfaces of different colours.

Some insects, such as honey bees, can be trained to search for food on almost any particular kind of coloured surface.

and with these a much fuller knowledge of their colour sense can be obtained. Bees, it is found, are sensitive not only to all wavelengths of light visible to man, but also to a wide region of the ultra-violet. They can distinguish this region of the spectrum as a separate "colour", as well as separating three distinct regions of the normal visible spectrum. As in man, orange-reds are complementary to blues, and for the bee greens are complementary to the ultra-violet region. Because of this bees can distinguish clearly between two types of white paper (identical to human eyes), one of which reflects ultra-violet strongly while the other absorbs it.

Research of this sort demands considerable imagination on the part of the experimenter who must try to "see" with the insect's eye, and not expect that this view of the outside world will necessarily correspond with his own.

Science for the Citizen

THE liaison between scientists and publicists of science, both actual and potential, is no sturdy plant. It is in sad need of nourishment and discussions such as the one organised by the London and South Eastern Counties Section of the Royal Institute of Chemistry on October 20 help a great deal. The subject before the well-attended meeting was "The Publicity of Science". The Press, films and radio received attention, and also exhibitions, a medium which is too often overlooked when considering scientific publicity.

The speaker on science and the Press was Dr. O. J. R. Howarth, secretary of the British Association, who considered that the provision to meet the increasing public interest in science is inadequate. He added that in this connection education had not caught up with the interests of the people. Gifted exponents to the lay public are uncommon among scientists, and among journalists those of scientific training sufficient to fit them as scientific correspondents are as yet not numerous. He spoke of the American Science Service, with its syndicated news, as a strong and efficient organisation, but pointed out that the structure of the Press in Britain and America (the field for syndication in the States is wide) made it possible to assert, justifiably or not, that in this country there would be insufficient support for a separate science news agency. A practical view of this question was opportune, but he thought the initiative should come from the Press. A strongly backed request for help in providing authoritative scientific news ought not to find science unresponsive.

Mr. O. F. Brown, chief information officer of the Department of Scientific and Industrial Research, talked on exhibitions, which he recommended as the most satisfying of all publicity media, at any rate from the point of view of presentation. A good exhibit must do three things: attract attention, hold interest, and leave a lasting impression. This maxim applies to the exhibit as a whole and to its component parts. Mr. Brown then spoke of the importance of achieving dignity and attractiveness in line and colour scheme, but added that the design should be judged by the extent to which it fulfils its function. The general layout should aim rather at the style of a Bond Street shop than a bargain basement. He instanced his remarks by reference to the Steel Hall at the Glasgow Empire Exhibition, and mentioned the use at D.S.I.R. exhibitions of short loops of silent film (120-150 feet long) to illustrate laboratory processes. Dioramas he considered attractive but rather unduly expensive for a short exhibition. In conclusion, Mr. Brown said he would like to see some informal society or group where people interested in scientific publicity could meet regularly and exchange views.

A strong case for the use of film was made by Mr. G. A. Jones of the Scientific Film Association. He pointed out that the average boy who leaves school at the age of 14 has by the age of 25-30 spent more time in a cinema that he did at school. The regular impact of the medium, he claimed, was far more powerfull than the spoken word alone. Yet this average boy has not been informed by film on any point of science. His mental picture of the scientist is probably a mysterious figure—akin to the Medicine Man of Africa—who lives a secluded life among "chemicals", glassware and sparking electrodes, evidently making magic. He does not connect science in any way with the clothes he wears, the food he eats or any other of the other facts of daily life that depend so much on science. Mr. Jones referred to the growth of the scientific film society movement, and to the fortunate fact that many so-called documentary films have dealt with the relation between science and our daily lives. Scientific films are of value to scientists themselves as well as to the public. "We need" said Mr. Jones "interpretative films to bring home to scientists the wider part they play. The place of film in the publicity of science will not have been filled until it is recognised, by scientists and non-scientists alike, that scientific method can and must be applied to matters far outside those covered in the science text-books".

The speaker on broadcasting was Mr. Vincent Alford, acting assistant director of talks at the B.B.C., who made high claims for radio—"the globe-spread net of speeded intercourse". He said it had the unique advantage of bringing the thoughts, words and personality of distinguished men and women into the listener's home. He invited his audience to think of the listener as a target, who would choose radio as the most desirable form of bombardment since it involved no more trouble than to stretch out an arm from his chair to turn a switch. Mr. Alford described the technique adopted in Nesta Pain's programme *What is Man?* and said that the medical case histories included in it were actual cases. The method substituted for the past tense of the text-book the present tense of the playlet. For both "talks" and "features" the final word must rest with the producer who understands the limitations of the non-expert in the audience. He suggested that it was easier to ensure a good broadcast on a physical or biological subject than on chemistry, a pre-requisite to a worthwhile talk on chemistry being a certain basic vocabulary among listeners. Mr. Alford recognised the difficulty of disarming the scientist's fear of publicity, but pointed to the holding of the conference as a sign that men of science were waking up to their responsibilities. "It is clearly becoming recognised that no effort is too great for scientists to take to render themselves intelligible."

The lectures were followed by a lively discussion. The chemists who took part were almost unanimous in taking a jaundiced view of the Press, and one speaker went so far as to advocate peace-time censorship of scientific news with the aim of ensuring greater accuracy. Censorship for accuracy has been attempted in Britain during the war (though *officially* such censorship never existed) but it was a conspicuous failure, as Mr. William E. Dick, editor of DISCOVERY, pointed out to the meeting. He made specific reference to the penicillin censorship restrictions in this connection. It was evident from the discussion that the scientist likes to see science get publicity on the grand scale, but it was implicit in many of the remarks made at the meeting that the scientist should "vet" (if not dictate) the manner in which science should be presented. It is on this issue that a state of deadlock is liable to develop. The conference ended on the right note, however, with Mr. A. L. Bacharach saying in summing up the discussion that it is more important for the public to get the right *impressions* about science. Details should be accurate, but it is unlikely that the public at the moment can appreciate or assimilate more than a very small proportion of them. The suggestion to form a Guild of Science Writers was mentioned, and Mr. Bacharach referred to another move, to form a science section within the Society of Authors, Playwrights and Composers. Similarly indicative of the prospects of action crystallising out of a mass of good intentions was Mr. Brown's suggestion that a group should be formed to exchange views on scientific publicity.

Supersonic Ranging by Bats

BATS during flight produce an interrupted supersonic tone (analogous to the pusles sent out by radar transmitters) which is reflected from neighbouring objects. Using their ears for stereophonic reception, bats can localise the positions and distances of these objects, and their nature, and can control their flight accordingly. These points were made by Professor H. Hartridge, F.R.S., in the course of a lecture to the Linnean Society last month, which has since been published in *Nature* (October 27, p. 490). As long ago as 1920 Professor Hartridge suggested that bats guided themselves by some such mechanism, and D. R. Griffin and R. Galambos, two American workers, have since carried out a systematic investigation of the phenomena connected with the flight of bats, their results, which fill in the details of Hartridge's hypothesis, being published in the *Journal*

of Experimental Zoology in 1941 and 1942. Professor Hartridge gave the duration of a bat's supersonic "pulse" as being about one-millionth of a second, and said that the number of pulses emitted per second was 12-60. The wavelength of the supersonic tone is 0·7 cm. (For radar the wavelength is 0.9 cm. and the number of pulses per second 200—20,000.)

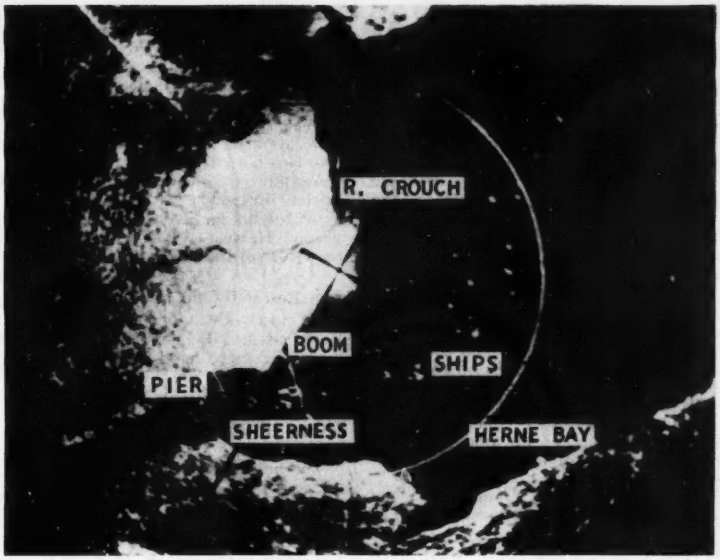
R.A.F. Radar Training

It is now disclosed that the training of thousands of airmen and women was organised from Reading University's Wantage Hall, war-time headquarters of R.A.F. Technical Training Command. At the beginning of the war there was only one training school for R.A.F. radar and the authorities, realising what the consequences would be if that were wiped out by enemy bombing and recognising the need for expanding training facilities, started new radar schools in both Britain and Canada. The original school was moved in the early months of the war from Bawdsey Manor, Suffolk, to Yatesbury in Wiltshire, where more than 25,000 men and women were trained on ground station work. 26 well-known technical colleges were eventually brought in to provide initial courses for recruits, tuition being given at Battersea, Belfast, Birmingham, Bolton, Bradford, Dagenham, Dudley, Edinburgh, Glasgow, Holloway, Hull, Leeds, Leicester, Lewisham, Lincoln, Liverpool, Londonderry, Manchester, Rotherham, St. Helens, Shrewsbury, Stockport, Walthamstow, Woolwich and Wolverhampton. The Sir John Cass Technical Institute also took part in the scheme. In Canada fifteen universities gave radar instruction.

Science and Middle East Supply Centre

The Middle East Supply Centre which was set up in April of 1941 came to an end at the beginning of this month. The Centre, which in the words of the British Foreign Secretary "made so signal a contribution to solving the problems of supply and distribution in the Middle East during the war", put into operation a great deal of scientific knowledge: scientists were brought in to assist in solving problems associated with agriculture, fishery development, public health and locust control, for example.

The systematic war that was waged against the locust has yet to be described in detail, but the following particulars are of interest. An Anti-Locust Unit was set up in the Middle East under the Supply Council to fight the pest over the whole area and to track down the insect to its breeding grounds. To meet the threat of a plague of locusts the Governments of Britain, the United States, Russia, India, Egypt, Palestine, Syria and Persia, the British military administrations in occupied territories and particularly the British Army all contributed to build a fighting force that was well equipped. Even aircraft were used. Mechanised expeditions were sent to remote corners of Arabia, Persia, Ethiopia, Eritrea and the Sudan. As a result of the concerted efforts one of the most serious plagues



THIS MONTH'S COVER—H2X, an improved version of H2S (see Discovery, September 1945, p. 288, Fig. 10), works on even shorter wavelengths and was responsible for this radar picture, taken at night, through thick cloud, of the Thames estuary. It shows Southend Pier, an anti-submarine boom and shipping.

ever to threaten the Middle East was completely defeated.

German Scientists Work in U.S.A.

The U.S. War Department has announced that a project has been approved whereby certain outstanding German scientists and technicians were being brought to the United States to ensure that full advantage be taken of those significant developments which were deemed vital to American national security.

Interrogation and examination of documents, equipment and facilities in the aggregate were but one means of exploiting German progress in science and technology, it was stated. In order that the United States might benefit fully from this source a number of carefully selected scientists and technologists were being brought to the United States on a voluntary basis. These individuals had been chosen from those fields where German progress was of significant importance and in which these specialists had played a dominant role.

Throughout their temporary stay in the United States these German scientists and technical experts will be under the supervision of the War Department but will be utilised for appropriate military projects of the Army and Navy.

A scientist reported to have gone to America is Otto Hahn, discoverer of the phenomenon of uranium fission.

Radar in Merchant Ships

A specification for a general purpose radar set that will increase the safety of Merchant Navy navigation has been approved and the Ministry of War Transport has communicated this specification to manufacturers for guidance in producing sets suitable for merchant ships. Advice on the technical aspects of the specification is also being made available to manufacturers by the Government. Surplus Admiralty sets are being released for the Merchant Navy, and other forms of radio navigational aids are being investigated by the Ministry of War Transport with the assistance of a scientific advisory committee.

U.S. Geologists in the War

The contribution of geologists to the British war effort was described in an article by Dr. W. D. Evans published in our last issue. On the other side of the Atlantic an interesting report of the work done by American geologists has been published. It is an official document prepared by the director of the U.S. Geological Survey.

Minute details of terrain vital to Allied military operations were collected by a Military Geology Unit comprising 66 men and 14 women geologists. This unit produced 140 geological "monographs" for the use of the American services.

The Military Geology Unit furnished the engineers with complete folios covering every operation in which American troops took part or planned to take part, with the exception of the Normandy landings for which the work was done by British geologists. The document states that when preparing for the assault on the Gilbert Islands it was found that no reliable geological information was available, but this apparently was the only serious "blind spot" encountered in the whole of this geological survey work.

At Leyte geological intelligence was lacking at the start of the landing. Engineers, forced to operate on the basis of what they could see above ground, were about to start building an important airfield when an army geologist arrived to inform them that the field chosen would be flooded during monsoons. Studies made in the United States had suggested that an area at Guivan would be most suitable for the purpose, and when the Japanese had been cleared from that area an airfield was built there. It proved to be one of the most important in the entire Pacific.

During the Sicilian invasion planes from a hidden German airfield were bombing the American lines constantly. Intelligence officers of the U.S. air command consulted folios prepared by geologists for the invasion in which were listed all possible sites suitable for airfields. The sites were reconnoitred from the air, and the Luftwaffe airfield was soon discovered and bombed out of action.

Soviet Academy's New President

SERGEI VAVILOV has succeeded Vladimir Komarov as president of the Soviet Academy of Sciences. Born in Moscow in 1891, Vavilov graduated from Moscow university in 1914 in mathematics. During the first World War he served in the Russian Army in a wireless battalion. In 1919 he began his professional activities at the university and at the Moscow Technical School. From 1918 to 1930 he directed the Department of Physical Optics at the Physical and Biophysical Research Institute of the People's Commissariat of Health. He was elected a corresponding member of the Academy of Sciences in 1930 and a full member in 1932.

In 1931 he was appointed scientific director of the State Optical Institute and in 1932 director of the Lebedev Institute of Physics in the Academy of Sciences.

Apart from a number of papers on wireless published during the first World War Vavilov's early work was concerned with the photometry of coloured light sources and the fading of paints. No better background could have been found than this for his most important later work on the fluorescence of dyestuffs in solution. He was a pioneer in this field, his school receiving world wide recognition. It is interesting to note that some of his first papers on the subject were published in the British *Philosophical Magazine*. His measurements of the intensity of the very weak light sources which occur in luminescence research led him to consider the ultimate sensitivity of the human eye, and in 1933, simultaneously with other workers in England and on the Continent, he came to the startling conclusion that the eye was so sensitive that it could detect fluctuations in the number of photons, or elementary light particles, in a beam of low intensity. This work on the one hand confirmed the quantum nature of light, and on the other contributed substantially to the advancement of physiological optics.

He has also published papers on the luminescence of crystalline solids and on the nature of the luminescence excited by gamma rays from radioactive materials.

The high esteem in which Newton is held in the Soviet Union has been fostered by Vavilov's books and articles and by his translation of several of Newton's works into Russian.

Vavilov is a member of the Supreme Soviet. In 1943 he was awarded the Stalin Prize for his work on luminescence and vision. He wears two Orders of Lenin and the Order of the Red Banner of Labour.

New Research Chief at Admiralty

THE post of Deputy Controller for Research and Development in the Admiralty changed hands last month, when Dr. C. F. Goodeve, O.B.E., F.R.S., pioneer holder of the appointment, left the department to take up the directorship of the British Iron and Steel Research Association.

The holder of the post not only helps the controller of the Navy in supervising the application of science, technology and design engineering in the large range of departments which work under his superintendance but is also adviser to the Board of Admiralty on research and development generally.

The new Deputy Controller is Mr. A. P. Rowe, C.B.E., who has during the war been head of the largest radar research establishment in the country, the Telecommunications Research Establishment at Malvern

Scotland's Midges

SCOTLAND'S Scientific Advisory Committee considers the midge problem (which was referred to in a letter published in the March issue of DISCOVERY) to be sufficiently serious to merit the appointment of a special sub-committee to investigate possible methods of midge control. This sub-committee includes the following zoologists and entomologists: Brigadier F. A. E. Crew (Chairman); Dr. A. E. Cameron, Edinburgh University; Dr. J. A. Downes, Glasgow University; Dr. Guy D. Morison, Marischal College, Aberdeen; Professor A. D. Peacock, University College, Dundee.

British Electron Microscopes

THOUGH pioneer work in the design of the electron microscope was done in Britain, the concentration of British scientists on problems connected with the war meant that the commercial production of the instrument had to be delayed. The research department of the Metropolitan-Vickers Electrical Co. of Manchester recently announced that among the special scientific equipment which it has under construction there are several electron microscopes arranged with provision for the subsequent addition of electron diffraction apparatus. This equipment is intended for investigation of the sub-microscopic structure of a wide range of substances, magnifications of the order of 10,000 diameters being obtained by means of the instrument, whilst the resolution is such that further magnification is possible by subsequent photographic enlargement of the electron micorscope image.

Natural and Social Sciences

THE Association of Scientific Workers is setting up a Joint Sciences Committee consisting of workers from various fields, who will consider the best ways in which natural and social scientists can be brought together to deal with social problems.

London Scientific Film Society

THE season of the London Scientific Film Society will consist of four or five shows on Sunday afternoons at 2.45 p.m. at the Scala Theatre, Charlotte Street, London, W. 1. Applications for tickets should be sent to the L.S.F.C. c/o The Scala Theatre. During the season it is hoped to show the following films: *Gen on GEE*, *Night Vision*, *Story of DDT*, *Take Your Chance* and *Temperature, Pulse and Respiration*, among other films. The first film show will be on December 9.

A Computing Laboratory

A COMPUTING laboratory was recently established at Columbia University. The new laboratory, known as the "Watson Scientific Computing Laboratory at Columbia University", will serve as a world centre for the treatment of problems in the various fields of science, whose solution depends on the effective use of applied mathematics and mechanical calculations. Dr. Wallace J. Eckert, formerly professor of astronomy at Columbia, is its director.

Personal Notes

THE death was reported, at the last meeting of the Linnean Society, of PROFESSOR G. HABERLANDT, the famous German plant physiologist.

DR. L. A. BORRADAILE, lecturer in zoology at Cambridge University and known to all medical and biology students in Britain for his *Manual of Elementary Zoology*, died on October 20, aged 73.

MR. J. STEWART COOK has been appointed as organising secretary of the British Association of Chemists.

PROFESSOR R. P. LINSTEAD, D.Sc., F.R.S., has been appointed Director of the Chemical Research Laboratory in the Department of Scientific and Industrial Research.

DR. W. F. P. MCLINTOCK, D.Sc., F.G.S., has been appointed director of the Geological Survey of Great Britain and Museum of Practical Geology. He has been deputy-director since 1937.

MR. ROY INNES, B.Sc., has taken up his duties as general secretary of the Association of Scientific Workers, in succession to MRS. REINET FREMLIN, who has held that position for the past 8 years, during which time she has seen the membership grow from 1,000 to 17,000.

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